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HISTORY, SOCIAL STUDIES, AND CITIZENSHIP: THE RESPONSIBILITY OF THE PUBLIC SCHOOLS

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(*Read April 22, 1960, in the Symposium on Current Issues and Readjustments in American Education*)

THE schools of a nation exist not only for the benefit of the individuals who attend them, but, equally important, for the welfare of the community that supports them. The raising up of a loyal, well-informed, thoughtful citizenry is a fundamental purpose of public education. Upon history and the social studies, among all the disciplines of the school curriculum, falls the primary responsibility for civic training.

Three different—and in certain respects, conflicting—tasks are involved. To become a *loyal* citizen means to identify oneself with the traditions, to share the ideals and assumptions, and to acquire the procedural habits that underlie the civic life of the community. To become a *well-informed* citizen means to assimilate the wide range of exact knowledge that alone can clarify the complex problems arising in the realm of governmental affairs. To become a *thoughtful* citizen means to develop the analytical and critical processes of mind that a man or woman must employ in making responsible decisions about public policy.

Though these three tasks are but parts of one continuing educational objective, they tend to follow one another in a natural sequence, dictated by the successive phases in the normal unfolding of the young mind. Alfred North Whitehead has perceptively described this development from infancy to maturity in his essay on "The Rhythm of Education." First comes a "stage of romance," discursive and "dominated by wonder." Next, for the young adolescent, must come a "stage of precision," keyed to "the inescapable fact that there are right ways and wrong ways, and definite truths to be known." Finally comes a "stage of generalisation," the business of which is "to convert the knowledge of a boy into the power of a man." At this point the student returns once more to "the discursive adventures of the romantic stage, with the advantage that his mind is now a disciplined regiment instead of a rabble." The educational cycle is thus completed. Studies "begin by evoking initiative and end by encouraging it."[1]

The sequence of tasks to be accomplished by instruction in history and the social sciences matches step by step the sequence of stages that Whitehead has traced. At the stage of romance the child drinks in the experiences that will make him a loyal citizen. At the stage of precision he masters the exact knowledge that will make him a well-informed one. At the stage of generalization he develops and applies the analytical powers of mind that will make him a thoughtful participant in public affairs. No hard-and-fast lines mark off these various stages, of course, and the chronological limits of each stage differ not only for different subjects (as Whitehead points out), but also for different individuals. Nevertheless, for history and the social sciences one may roughly equate the stage of romance with the first six years of the elementary school, the stage of precision with the next five years (that is, the span covered by the junior and most of the senior high school), and the stage of generalization with the final year of high school and the four years of college.[2]

The gravest weaknesses of the American public-school system are to be found in the years that are devoted—or should be devoted—to the tasks belonging to the stage of precision. In the United States, indeed, a stage of this sort seems to have totally disappeared, especially from the instructional program in history and the social sciences. Discursive activities appropriate to the elementary

[1] Whitehead, Alfred North, *The aims of education & other essays*, 43, 50, 54, 57–58, New York, Macmillan, 1929.

[2] Whitehead considers the romantic stage to extend, generally speaking, until the age of thirteen or fourteen; the stage of precision from fourteen to eighteen; of generalization from eighteen to twenty-two. *Ibid.*, 59. On the other hand, recognizing a difference among academic disciplines, he speaks of the stage of precision for language and literature as beginning about the age of eleven, and that for science at about fifteen. *Ibid.*, 35–37.

school are prolonged into the high school. Then, before systematic study of history or of any one of the social sciences can begin, students are turned loose to engage in another round of discursive activities, purporting to acquaint them with the problems of contemporary American democracy. The notion that they are tackling these problems at a mature level is sheer illusion. In fact, they are tackling them at an infantile level, for no intervening stage of precision has furnished them with the systematic knowledge and the tested intellectual power prerequisite to adult thinking. They have missed the vital training that should have turned their minds, as Whitehead phrases it, into "a disciplined regiment instead of a rabble."

In many fields of knowledge, high-school students are missing the indispensable training in precision simply because they are allowed to omit the subjects in question from their programs of study. Insufficient attention to mathematics, science, and foreign languages is reflected, quite simply, in the figures for enrollment in courses in these fields. Statistics, however, do not reveal the shortcomings of high-school instruction in the two fundamental areas of English, on the one hand, and of history and the social sciences, on the other. Because the subjects are usually required, course enrollments are large. Alarming weaknesses appear, however, when one begins to look beneath course labels and to observe how the content has been curtailed and disorganized. The widespread illiteracy that has resulted from deficiencies in public-school programs in English composition and literature is apparent. Less well known, but even more menacing, is the situation in the social studies. Disintegration within this area of the curriculum poses a direct threat to American institutions of self-government, which demand of the citizen both a knowledge of history and a developed ability to subject public issues to rational analysis.

The first symptom of disorganization is the intrusion into courses in the social studies, so-called, of utterly extraneous subjects of instruction, bearing no conceivable relation to the development of a loyal, well-informed, thoughtful citizenry. In California, for example, state law requires the schools to furnish thirty hours of instruction in driving an automobile. How have school administrators handled the requirement? I quote from the *California Journal of Secondary Education*:

> By far the most common solution to this curriculum problem has been the placement of thirty hours of driver education instruction into conventional subject courses. Since this injection has been made most frequently into a social studies course it has meant the necessity of either condensing or eliminating certain previously taught subject matter.[3]

Let us look more closely at this matter of driver training. Admittedly no one is acting as a responsible citizen if he drives an automobile recklessly down the public highway. But such a judgment does not convert the operation of a vehicle from a mechanical skill into an intellectual discipline, nor does it make driver training one of the social sciences. The absurdity of accepting driver training as a substitute for history or political science in the education of citizens can be made apparent simply by suggesting the converse: that a driver's license be automatically issued to every student who demonstrates a satisfactory knowledge of the American constitution. Let us, by all means, enforce high standards of competence for those who seek the privilege of driving on our highways. But let us not do so by lowering the standards of competence for those who in the future will hold the steering wheel of the Republic itself.

Second among the disruptive factors at work today is an obsession with "life adjustment." This has turned many a social-studies classroom into a clinic for discussing the personal problems of adolescents. Professional psychological and psychiatric counselling should certainly be available in or through the school, but to furnish it is neither within the competence of the ordinary teacher of history or the social sciences nor within the province of courses in these subjects. The concern of the latter is with public and civic affairs. Knowledge of the organized institutions of society, and ability to analyze their workings, will undoubtedly enable men and women to live their private lives more effectively within the social framework. This result, however, is a by-product not a direct purpose of instruction in history and the social sciences. The ultimate aim of these disciplines is to develop *civic* understanding and competence, not to disseminate helpful counsel on intimate personal problems.

The social-studies curriculum has been warped, in the third place, by an all-too-exclusive preoccupation with contemporary affairs. No one

[3] Farris, Ragene A., How secondary schools are meeting education code requirements, *Calif. Jour. Secondary Education* **30**: 295, 1955.

would deny—certainly I would not dream of denying—that education should prepare young people (and prepare them thoroughly) for dealing with the public problems that will confront them as adult citizens. This cannot be done, however, by focusing their attention in the classroom simply on the problems of the present moment and treating all else as "background." It is fallacious, to begin with, to assume that there is such a thing as a strictly contemporary problem. Even were a problem to burst upon us today without precedent or warning, it would by next week have become in part an historical problem. Actually, of course, economic and political issues do not suddenly appear out of nowhere. They arise slowly and they alter and develop over long periods of time. To understand how they came into existence is usually the most important step in grappling with them.

One must remember, moreover, that young people are to function in the world of tomorrow, not the world of today. They must accordingly understand the inescapable fact of social change, which only history can really teach. Future citizens must be capable of making wide-ranging comparisons between past situations, decisions, and events, and those that newly arise in their own day. They must be acquainted with forces that are perhaps temporarily inoperative but that have exerted a powerful influence upon times past and may upon times future. If students grow up in a period of peace, for example, they must nevertheless know what war is. Their experience of many things must be gained vicariously—through historical study—if they are to face a future about which nothing is known for certain except that it will differ from the present. Preoccupation with contemporary affairs, in programs of social studies, deprives young people, in effect, of the ability to profit from the whole past experience of mankind.

Ill-considered innovations sponsored by educational theorists have combined with misguided pressures arising outside the schools to turn the social-studies curriculum, in many school systems, into a mere catch-all. Take, for example, the following description offered without apology by the principal of a California high school:

> Our present plans call for three problems courses during a student's high-school career: (1) Freshman problems with the 10 units of work covering the State requirements of Morals and Manners, Civic Responsibility, Alcohol and Narcotics; (2) Sophomore problems with 5 or 6 units of work covering Driver Education, First Aid, Accident Prevention and World History; and (3) Senior Problems with units of work covering Civics, Psychology and Sociology and other problems facing the 18-year-old.[4]

Can anyone possibly believe that loyal, well-informed, thoughtful citizens will be produced through study of such a hodge-podge of "problems"? To treat first aid and driver education as social sciences, to reduce world history to a mere unit alongside a companion one in accident prevention, to try to inculcate civic responsibility in a context of utter intellectual irresponsibility—such procedures cannot be represented as a new organization of the social studies. They constitute a deliberate, total, and menacing *dis*organization of the curriculum in one of its most vital areas.

Between the social studies, so-called, in elementary and secondary schools, and the social sciences as known to the mature world of scholarship, there exist at present only the most superficial resemblances and only the most tenuous intellectual connections. History, political science, geography, economics, and the other social sciences are organized disciplines, not random collections of more or less entertaining facts. As disciplines they stand for the orderly investigation of social relationships and human occurrences, for intellectual analysis, for systematic comparison, for theoretical understanding as a basis for practical action. Public-school courses in the social studies hardly pretend to offer methodical instruction in the precise and powerful intellectual tools employed in the various social sciences. Instead they are likely to consist of a succession of unrelated topics, selected not because they will systematically develop the intellectual powers of the student, but because they will presumably appeal to his evanescent "real-life" interests.

Pupils in public-school social-studies courses are expected to acquire a sense of history through casual encounters with snippets of historical data, presented as background to one or another contemporary problem. They are expected to pick up the principles of the other disciplines by noting

[4] Waller, Lloyd, quoted *ibid.*, 295–296. While the present paper was being prepared for publication, *The New Yorker* picked up the following from a teachers' guide issued by the Pasadena City Schools: ". . . in the tenth grade study is concentrated on the growth of Democracy, and especially the form of Government which developed. Such a study should be brief and to the point in order to allow time for the unit on Driver Education." *The New Yorker* **36** (14): 93, May 14, 1960.

such theoretical questions as may crop up, from time to time, in "problem-solving" situations. Though courses in the social studies may examine problems with economic aspects, they do not teach economics as the economist understands the subject, that is, in terms of coherent theory. Though they may take note of historical "backgrounds," they do not teach history in the historian's sense, as a long development in time, involving constant interaction among contemporaneous events of various kinds. In the social studies, so-called, field trips enjoy a peculiar vogue. If students visit battlefields and the birthplaces of famous men, they will absorb history painlessly. If they see enough factories in operation they will understand economics. If they shake hands with the mayor in his office they will know political science. Work in the classroom is similarly episodic. Students are expected to take up one contemporary problem after another, observe its bewildering complexity, provide some glib answer to it, and pass on.

Where the social-studies approach is pushed to its logical extreme, graduates acquire a smattering of information about everything in general, but a clear and usable knowledge of nothing in particular. Pupils may have visited countless public institutions and governmental offices on field trips, but they have not developed a consistent philosophy of constitutional liberty that will stand up against the arguments of a determined and skillful opponent or demagogue. The false deference paid in the classroom to their snap judgments on difficult contemporary issues—political, diplomatic, economic, or social—has not taught them to *study* contemporary problems. Quite the reverse. It has implanted in them the arrogant and fatal belief that they can deal successfully with contemporary problems by a round of group discussions, without benefit of precise knowledge, logical analysis, or historical understanding. Once a disciplined approach to knowledge disappears, the social studies become a maze of dead-end streets. As offered in many American schools today, they provide no cumulative knowledge, they lead to no over-all conclusions, they lay no foundation for advanced study, they generate none of the intellectual competence prerequisite to mature citizenship.

In defense of this chaos, educationists invoke the words *synthesis* and *integration*. They are applying to the lower schools, they insist, the same principles of interdisciplinary teaching and research that have proved fruitful in colleges and universities. The analogy is totally false. To deal with a particular problem at a research level, the resources of several disciplines may often be usefully focussed upon a single point. The value of interdisciplinary research, however, derives from the fact that each discipline has developed—by virtue of being an independent, organized discipline—certain powerful tools of its own, complementary to, but different from, the tools of its sister disciplines. Until these various methods have been mastered, each in its own terms, the interdisciplinary approach is sheer illusion. The power to generalize presupposes a power to analyze. But the public school is not requiring its pupils *first* to master history and economics and *then* to apply their systematic knowledge to the analysis of a problem that is both historical and economic in nature. Instead, some of the most complex problems that humanity has ever faced are laid before pupils, who are then supposed to attack them with bare hands and even barer minds.

In support of this procedure, educationists often blithely argue that a student learns more by solving a contemporary problem than by studying one that time has already solved. This statement is utter nonsense. A student does not *solve* a contemporary problem in the classroom. At best he merely clarifies his own opinions—or, more likely, borrows his teacher's—as to the way a given problem might or ought to be solved. A student has no power to put his "solution" to the proof of action. He cannot test it by experience or by results. No conclusion he reaches can be shown to be right or wrong until after the returns are in, perhaps long after he is dead. The logic of any particular argument may look sound enough on paper, but only the future can tell whether the student has estimated correctly the factors involved, or whether he has omitted from his calculations certain historical forces that will in the end prove decisive. The long-term results are far beyond his knowing and that of his teacher. A man must often *act* on imperfect knowledge, but imperfect knowledge is a poor thing from which to *learn*.

Actually it is the study of history, not the study of contemporary issues, that provides genuine "problem-solving situations." History, to the historian (and to the young student if the discipline is properly taught), is nothing other than a series of problems—problems of weighing evidence, problems of generalizing and checking the validity of the resulting generalizations, problems

f attaching the proper weight to various factors
n a given situation, problems of exercising judg-
ient concerning the probable outcomes of various
nes of action. History provides training in
recisely those processes of mind that a citizen
ust use when wrestling with the problems of
is own day.

The study of history has one overwhelming
dvantage over the study of contemporary prob-
ms. The results are in. There exists, in a
ense, an answer sheet, against which the student
an check the accuracy and adequacy of his an-
lyses and his judgments. He enters into the
roblem as contemporaries entered into it. But
e has access to knowledge that contemporaries
ould never attain. He knows the consequences
f the lines of action proposed and pursued. He
nows what finally happened.

The student schooled in this way has learned
he limitations of contemporary judgment. He
as taken a long view, consequently he knows
hat a long view means. He understands how
mportant it is to reckon with the unexpected.
Ie comprehends the dismaying fact that par-
icular programs and points of view, logical and
enign on paper, may turn into barbaric fanati-
isms when their advocates are entrusted with
ower. Banished from his mind, we may hope,
vill be the half-baked arrogance of those who
elieve they have "solved" a contemporary prob-
em on their little slates. In its place, we may
rust, will have come wisdom, and its concomitant,
ntellectual humility.

Fortunately, many American young people con-
inue to be schooled in this way. Common sense
nd sanity prevail in large numbers of schools
vhich have never subscribed to the fantasies of
he social-studies movement. These continue to
ffer straightforward programs in history, sup-
lemented by courses in American constitutional
rinciples, often under the name of civics. Ex-
ellent models thus exist for a restoration of the
urriculum to what it should be. Such models are
o be found in the offerings of the best schools
f the United States, public and private, in the
rograms of European school systems, and in the
ecommendations of various committees of his-
orians and political scientists, particularly those
ublished in the 1890's before the disintegrating
endencies of recent decades became so powerful.[5]
Though sound in principle, these great reports of two generations ago are out of date in detail. For the latter part of the twentieth century we need a fresh scholarly appraisal of, and a fresh set of scholarly recommendations for, the American high-school curriculum—especially the program in history and the social sciences.[6] I emphasize the word scholarly. The views of professional educationists are already spread upon the record. The various social-studies programs I have discussed represent a carrying out, in whole or in part, of their views. It is time for the views of others to be independently presented. A commission made up of historians, of scholars in various of the social sciences (especially geography, political science, and economics), of members of the bench and bar, and of experienced public servants would possess the desired qualifications. Men in such positions are aware, as specialists in pedagogy cannot be, of the intellectual demands that the modern world is actually making, and will increasingly make, upon the adult citizen.

Prerequisite to any sound planning must be a clear comprehension of the nature of history and of the various social sciences, considered as intellectual disciplines. From this point of view, the differences among them are as significant as the resemblances. Though history provides vital data for most of the social sciences, and though it makes use of principles and concepts derived from each of them, it is not, in the profoundest sense, one of the social sciences. History is essentially a synthesizing discipline, concerned with explaining how forces of varied kinds combine to produce particular unique situations—situations that never recur in quite the same form. By contrast, each of the social sciences is essentially analytic, seeking to pick out, from a multitude of diverse situations, certain common elements and principles, the validity of which is, in some sense, universal. Each approach constitutes a check upon the other. History corrects the tendency of the social sciences to find in the contemporary situation a universality which it really lacks. The social sciences, in return, correct the defects that arise from the fragmentary nature of the surviving sources on which

[5] See the able discussion of these reports in Johnson, Henry, *Teaching of history*, rev. ed., 25–85, New York, Macmillan, 1940.

[6] An impressive start has been made in two recent cooperative studies by companies of academic scholars and scientists: Koerner, James D., ed., *The case for basic education*, Boston, Little, Brown, 1959; and Rapoport, Henry, William C. Bark, and others, Report of the San Francisco curriculum survey committee (mimeographed), San Francisco, Board of Education, 1960.

historical conclusions must depend. With respect to the segment of history which we call the present, the social scientist can obtain for himself at first hand certain kinds of evidence that are missing from most of the records the historian is obliged to use.

Though history and the social sciences are interdependent, solid reasons exist for making the study of history the foundation for study of the social sciences, rather than the social sciences the foundation for history. In the first place, the chronological unfolding of events is far closer to the actual experience of young persons, than are the processes of abstraction—often high-level abstraction—employed in the social sciences. The subject matter of history, moreover, is more varied and more inclusive than the subject matter of any one of the social sciences. Finally, there is educational significance in the fact that it is from history that many of the data of the social sciences come. Just as observations must precede theory-making in the natural sciences, so historical knowledge must in general precede theory-making in the social sciences.

A concentrated and systematic attention to history throughout most of the junior- and senior-high-school years would bring to an end, quickly and decisively, the chaos into which the social-studies curriculum has degenerated. History is an organized intellectual discipline, the same from the elementary school through the graduate college. When history is taught as such, the scholarly soundness of the program can be maintained through continuous checking, as it cannot be when ill-defined, eclectic programs labelled "social studies" are substituted. Teachers of history can be properly and adequately trained in a well-delimited field. Courses of study can be planned under responsible scholarly guidance. Pupils can learn not only from their actual instructors but also from textbooks, collections of sources, maps, and reference works prepared in collaboration with responsible professional historians. In a properly organized school program in *history*, the resources of an entire scholarly profession are ultimately available to every individual child.[7] To guarantee the integrity of teaching in this area, so "sensitive" and yet so vital to citizenship, the public schools should offer a systematic, wide-ranging, cumulative program in history—labelled as history, organized as history, taught as history, faithful to the standards of accuracy and objectivity that history imposes.

A continuous program in history, from the seventh through the eleventh grade, fits exactly the needs of what Whitehead has called the "stage of precision" in an adolescent's intellectual development. Permit me one more quotation from that philosopher:

> Now is the time for pushing on, for knowing the subject exactly, and for retaining in the memory its salient features. This is the stage of precision. . . . There is no getting away from the fact that things have been found out, and that to be effective in the modern world you must have a store of definite acquirement of the best practice. To write poetry you must study metre; and to build bridges you must be learned in the strength of material. . . .
>
> During the stage of precision, romance is the background. . . . But the area of precise knowledge, as exacted in any general educational system, can be, and should be, definitely determined. . . . A certain ruthless definiteness is essential in education. I am sure that one secret of a successful teacher is that he has formulated quite clearly in his mind what the pupil has got to know in precise fashion. He will then cease from half-hearted attempts to worry his pupils with memorizing a lot of irrelevant stuff of inferior importance.[8]

The last comment is peculiarly pertinent for history. When taught without a clear understanding of development and structure, history always appears to be an arbitrary collection of dates and names and events, to be memorized through a sheer act of will. When chronology is recognized as an organizing principle—as it is when history is studied in its own terms as an unfolding of events—dates and other details are absorbed as integral parts of the ordered body of knowledge which the mind, and not merely the memory, takes hold of. Admittedly an immense amount of data must be assimilated in the study of history—actually a far larger and more varied body of sheer information than the natural scientist needs to have at immediate command. To be useful at all, the facts must be reduced to comprehensible order in the mind of the student. Systematic organization, accordingly, is more essential in history and the social sciences than in any other field of instruction. Unless the body of knowledge is analyzed and ordered, learning turns into a prodigious feat of memorization, or else into a blind groping through a fog of diffuse abstractions.

[7] An admirable example of what can be done is afforded by the series of pamphlets (now numbering 30) that have been published since 1957 by the Service Center for Teachers of History, sponsored by the American Historical Association, Washington, D. C.

[8] Whitehead, *op. cit.*, 52-57.

These considerations set the terms of our problem. At the beginning of junior high school—the seventh grade—the time for precision has arrived. Much knowledge has been acquired discursively during the six elementary grades, some through first-hand observation, more through episodic reading and listening. The seventh grade is the place to pull this together as an ordered body of knowledge, and to fill the gaps that are found to exist. A course in American history, with considerable emphasis upon exact geographic knowledge, would be the appropriate introduction to the stage of precision.

The history of the United States should then be laid aside completely for the next three years. Under present arrangements, the same aspects of American history are brought up so repetitiously at successive stages of a child's schooling that interest (and consequently even understanding) are often destroyed by sheer monotony. A well-conceived, cumulative program of history can afford to cut loose, boldly and deliberately, from this nationalistic preoccupation, in order to give opportunity for a systematic three-year study of the history of the world outside our own boundaries. A sound program might well commence in the eighth grade with a course in ancient and medieval history, illuminated by constant attention to ideas, to literature, and to the arts. In the ninth grade a study of the history of Europe (including the history of England) from the sixteenth to the beginning of the twentieth century would be appropriate. The tenth-grade course might well be devoted to world history in the twentieth century, stressing international relations and paying careful attention to geographic elements. Such a course would be more manageable than the amorphous survey of world history, with vast chronological sweep, that is sometimes attempted. Opportunity should be given, in the course of this year, for a few retrospective excursions into the earlier history of certain selected non-European areas.

In the eleventh grade, students would return to the history of the United States, with both greater maturity and a vastly enlarged basis for comparison and evaluation. Specific attention might well be given to the foreign relations of the United States, to tie the course more closely to the world-ranging studies of the preceding year. Such an emphasis would also differentiate the course clearly from that of four years before.

The senior year of the high school is the place for systematic study of one of the social sciences, presented in its own analytical terms. Almost certainly the choice should be political science. Under such auspices, the course would examine the constitutional system of the United States with the thoroughness that existing laws supposedly (but often ineffectually) require. Comparisons with other governmental systems would naturally form part of such a course, and would illumine, from a new point of view, the historical studies of the ninth and tenth grades.[9] Possibly an elective course in the principles of economics might also be available to seniors. Geography should perhaps also be offered as a separate subject, though attention to geography in the courses in history should not be lessened. Systematic introductions to sociology or anthropology or social psychology, on the other hand, are best deferred to the university.

The systematic course in American and comparative government here proposed for seniors is by no means identical with the twelfth-grade "course on American problems or American government" recommended by Dr. James B. Conant in his report on *The American High School Today*. He envisages, not a methodical study of constitutional principles and political structures, but a series of class discussions of contemporary topics, particularly topics possessing an economic implication. The "free discussion of controversial issues should be encouraged." In this course, moreover, the groupings of students by ability would be deliberately abandoned in favor of classes each of which would be "a cross section of the school," the conscious aim being to develop "mutual respect and understanding between different groups of students."[10]

Though Dr. Conant is obviously endorsing the "contemporary problems" approach to the social studies, one should note that he confines its application to a single course. One can infer no endorsement of the reorientation towards contemporary affairs of the social-studies curriculum as a whole. Indeed, Dr. Conant recommends elsewhere a two-year requirement of unadulter-

[9] In the Report of the San Francisco curriculum survey, William C. Bark concurs with many of the points made in the present article, then proposes a slightly different (but quite acceptable) arrangement of courses. *Op. cit.*, 63–72.

[10] Recommendation 21, in Conant, James B., *The American high school today*, 75–76. New York, McGraw-Hill, 1959.

ated history for every high-school student,[11] and this must be considered a prerequisite for the senior program he proposes. If, in fact, the course in American problems could be made to represent a sharp break with the work of preceding years (not a mere continuation of earlier desultory wanderings), and if it could be conducted in such a way as to require each student constantly to bring forward and apply the information and skill systematically acquired through previous study, then such a culminating course could be defended as intellectually responsible.

Even so—even in the unlikely event that the prerequisite of two years of systematic historical study were enforced—grave doubts remain about the suitability of the proposed course. Its objectives are altogether admirable. Before they leave high school, students should be thinking clearly and arguing vigorously about the public issues arising daily in the adult society they are on the point of joining. They should be applying the lessons of the classroom to the affairs of the world outside. In discussions of this sort, moreover, students should meet on equal terms, without distinction of ability or status or specialized interest, for this is how they must thereafter meet on the common ground of citizenship. The exchange of ideas on controversial issues, finally, must be in an atmosphere of liberty, both of thought and expression. There must be a place for the free and vigorous discussion of controversial issues within the precincts of any academic community that is intellectually alive.

Is there no place but the classroom for such exchanges of opinion? Virtually all proposals for active student discussion of contemporary problems overlook the possibilities inherent in extra-curricular activities. This great area of effort and interest—potentially so powerful a contributor to the intellectual life of any school—has been almost completely surrendered to athletics and to the frivolities that make the social life of so many public schools (and even certain universities) a perpetual mardi gras. If the schools are really to accomplish what they ought in encouraging the free and lively discussion of controversial contemporary issues, they must recapture and redeem for serious intellectual purposes large sectors of extra-curricular time. New life must be breathed into the ancient art of debate. The assembly periods of the school must be consciously planned to deal with the affairs of the outer world, not the self-centered affairs of the school. Discussion sessions (heterogeneously grouped) should be arranged to follow each presentation of an important issue in a school assembly. Extra-curricular clubs should be organized for the informal study and discussion of such areas of interest as foreign affairs, economics, race relations, and politics. Leadership in such organizations (together with leadership in science clubs) should be rewarded by school authorities with honors at least equivalent to those so conspicuously bestowed upon athletes.

The classroom has its own tasks to perform, and these are exacting tasks. Discussion plays a role in effective instruction, of course, but its role there is clarification. New interpretations of the material in hand are brought out through discussion, and new sources of information are suggested. Even more important, misleading assumptions, inaccurate information, and faulty reasoning are revealed. The teacher's job is to question tendentiously, to expose fallacies, to correct errors. This role is the very opposite of the one that the moderator or chairman of an open forum is called upon to play. The teacher is obliged, by his office, to express judgment; the moderator, by his, to refrain from doing so.

In an open forum every man or woman is entitled to his own opinions and to respect for these. It does not follow (no matter how often the fallacy is asserted) that one man's opinions are as good as another's. An unconsidered, frivolous, off-the-cuff opinion is *not* as good as a reasoned, well-supported, responsible one. A first duty of the school is to make this distinction crystal clear, and the place to do so is the classroom. An atmosphere of freedom must, of course, pervade the school. Into the classroom, however, the irresponsibility of street-corner debate has no license to enter. *There* the teacher cannot be allowed to abdicate, in favor of a specious neutrality, his duty of giving judgment for knowledge as against ignorance, for reasoned argument as against prejudice, for intellectual seriousness as against frivolity.

In attempting to make the classroom a forum for the airing of opinions on controversial issues, the school does more than disrupt the orderly

[11] Recommendation 3, *ibid.*, 47. Conant's proposed graduation requirements for *all* students specify "three or four years of social studies—including two years of history (one of which should be American history) and a senior course in American problems or American government."

progress and the indispensable continuity of learning. It actually stifles the very freedom of expression it ostensibly seeks to advance. It does so by moving the discussion into an artificial and inappropriate environment. A hot-house course in contemporary problems gives no effective training in independent and active citizenship. We must encourage the initiative of students themselves, not depend on the directing authority of teachers, if we intend that principles learned through study and instruction shall be brought out from the classroom—literally, out from the classroom—and applied spontaneously to the issues of civic life.

Democracy rests upon the free exchange of opinions. It rests, no less, upon the patient, methodical, rational accumulation and utilization of knowledge. The American public school must teach respect for both. It cannot do so by confusing one with the other. Within the world which it creates for its students, the school must distinguish between the systematic study that constitutes its curriculum, and the free-ranging intellectual life that ought to pervade the whole institution, stimulating, but never disrupting, the work of its classrooms. The free expression of opinion is no substitute for the acquisition of precise knowledge, just as the acquisition of precise knowledge is no substitute for the free expression of opinion. The public school can encompass both, but only if it finds the means of creating, in the extra-curricular realm, a genuine forum for the discussion of controversial questions, and only if it preserves inviolate the original and indispensable dedication of its classrooms to thorough, accurate, and ordered learning.

SCIENCE EDUCATION IN THE ELEMENTARY AND SECONDARY SCHOOLS

JOHN R. MAYOR

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(*Read April 22, 1960, in the Symposium on Current Issues and Readjustments in American Education*)

PURPOSES

A STATEMENT of agreement among scientists and educational philosophers on the purpose of teaching science in the elementary and secondary schools would strengthen the encouraging cooperative effort of scientists and educators to improve science education at all levels. Science teachers and science textbooks often have not spoken clearly to students. In the widely discussed book by Stanislaus Joyce about his brother, James Joyce, a quotation from a report of an editor to whom James Joyce had sent a manuscript is given. The editor wrote:

> I have at last found time to read your play. It has interested me and puzzled me a good deal—indeed, I scarcely know what to say of it. You seem to me to have talent—possibly more than talent—and yet I cannot say that I think this play a success. . . . I cannot help finding the canvas too large for the subject. . . . I have been trying to read some elaborate symbolism into the second and third acts to account for their gigantic breadth of treatment, but if you had a symbolic purpose, I own it escapes me. It may be very good symbolism for all that—I own I am no great hand at reading hieroglyphics.

Reading these sentences reminds one of what some very competent scholars in other fields often have concluded about their high school experiences in the study of science and mathematics. They report their teachers had talent and that the subject seemed important, but that the purposes someway escaped them.

In an editorial, "A Model for Today," which appeared in *Saturday Review,* June 6, 1959, Mark Van Doren writes with great concern about the lack of communication between scientists and humanists. He states: "The gulf between the partners has produced illiteracy in both, but the mathematical illiteracy of the humanist is more serious in my view than the verbal illiteracy of the scientist." This particular sentence has an appeal for the mathematician. However, the fault for the resulting unhappy situation must be shared equally by the humanists and the scientists.

In *Science* for February 12, 1960, Bertrand Russell reemphasizes that the scientist can no longer shirk responsibility for the use society makes of his discoveries. This is an exceedingly important problem for the next decade. Russell makes clear that it is not new, in his reference to Galileo employed by the Grand Duke of Tuscany to calculate trajectories; the unhappy dilemma of scientists in the French Revolution, especially of Lavoisier, who was not spared because he was "only discovering" hydrogen; and Faraday, who informed the British Government in the Crimean War about the possibility of attack by poisonous gases, but refused to have any part of it. Are we preparing Faradays for tomorrow?

The Physical Science Study Committee (PSSC) suggests that a number of lessons are to be learned from the PSSC program. One of these is "high school students will respond to an intellectual presentation of subject matter in which rational thought and analysis are more important than brute force and memory." This might be said to be a primary goal of all of the current widespread efforts to produce new curricula in the sciences.

A NEW FORCE

The Association for Supervision and Curriculum Development in 1953 published a year book entitled *Forces Affecting American Education.* In this year book, and with accuracy in reporting, no reference is made to the interest of the community of scholars as a force affecting American education. In 1960 this interest appears to be the most important force of all in science education. Action associated with this interest will surely determine the direction of science education not only for this decade but also for the remainder of the twentieth century. In science education this force is best represented by the National Science Foundation sponsored curriculum studies: in physics, to which reference has been made; in mathematics under the School Mathematics Study Group; in geology by a group working at the University of Minnesota in Duluth in the summer of 1959; and in the efforts under way in biology and chemistry. Per-

haps the most important aspect of all of these efforts is that high school teachers and college teachers, including research scientists, are finding that they can work together and that the experience is stimulating to both. The development of science in this century is now so overwhelming that there seems no other way to plan science curricula for the schools. The success of the work in the sciences dictates that these kinds of efforts be extended to other areas, and with federal financial support.

In the academic year 1959–1960 the new physics course was taught in hundreds of schools to thousands of students, and for many it was the second year of teaching experience with the course. This year more than 600 teachers in all parts of the United States taught the sample textbooks produced by the School Mathematics Study Group for grades 7 through 12. In the summer of 1960 at Stanford University the mathematicians gave their attention to the development of a sample textbook for grade 4 and experimental units for grades 5 and 6. As illustration of the kind of teamwork in this production, one finds on the outline team for the elementary school work in mathematics research mathematicians, including a world-renowned research mathematician, working as team members with elementary schoolteachers.

Scientists and scholars are also working with teachers and school administrators in the development of new methods and administrative procedures which are often controversial in what might be called professional education circles. New York University with the support of a private foundation is conducting an experimental program on the use of special teachers of mathematics at the elementary level, and a somewhat similar program is being carried out under the auspices of the Science Teaching Improvement Program of the American Association for the Advancement of Science. In May, 1960 these two groups, with teachers and administrators from some twenty states, met in Washington to determine what has already been learned from these experimental studies and what problems need yet to be investigated.

TEACHER EDUCATION

There remains a very considerable controversy and associated dissatisfaction about teacher education. Speaking as a professor of education, part-time, at the University of Maryland and former chairman of a large department of education and a former acting dean of the School of Education in a midwestern state university, I would state with considerable conviction that we must have a new view of teacher education, in which either traditional professional education requirements are omitted, or the professional education sequence is completely reconstructed. There are two considerations which are cause of special concern:

(1) After thirty years in which professional Education (with a capital E) has been an important part of the curriculum of colleges and universities, Education has not been established as a respected discipline.

(2) There is a growing tendency among professional educators responsible for teacher education programs to increase the time allotted for student observation and teaching so that the four-year graduate may be more experienced upon graduation and hence do better in his first year of teaching in the schools.

There is a body of knowledge, represented not only by contemporary writing but also by the writing of philosophers throughout the ages which should be the basis for professional education. It is not clear why use has not been made of this work. A recent publication *Prologue to Teaching*, described as reading and source material, is a collection of writings on education from Aristotle and Plato to current times, including decisions of the Supreme Court of the United States. Competing points of view are presented so that the student may formulate his own philosophy of education. This book, edited by a Hunter College and a Western Reserve University professor of education, demonstrates the substantial discipline which professional education might become.

With many demands for study in the sciences, in the humanities, and the social sciences, and with the recognition by all, including the most excited defenders of professional education, that the four-year undergraduate program is not sufficient, it seems a most lamentable trend to increase at the undergraduate level the time for student observation and student teaching. Public schools cannot now expect the teacher with a baccalaureate degree to be an experienced teacher. It is their responsibility to provide in-service training during the first one, two, three, or four

years of teaching, which will make unnecessary the sacrifice of so much valuable time at the college level for student teaching and observation. I hope on another occasion to be able to develop this thesis at great length.

There has been sharply increased interest in in-service programs for teachers, which are led by scientists and which involve the study of science content. The provisions of the National Defense Education Act administered in the Office of Education, Department of Health, Education, and Welfare, make possible the use of federal funds through state departments of education for this purpose. It is to be hoped that a great many more schools will develop such in-service programs. The current interest in this kind of in-service training in subject matter is another testimony to the tremendous contribution of the curriculum studies. Teachers faced with the possibility of teaching the new physics or the new mathematics are exceedingly anxious to be better informed about content.

It does not seem proper in a paper before the American Philosophical Society on science education in the elementary and secondary schools to fail to give vigorous support for more adequate federal aid to education. It is clear that this aid is needed not only to stimulate work in the areas of special need, identified by federal government and exemplified in the programs of the National Science Foundation and the National Defense Education Act. More funds for the schools most certainly must be made available to the states to provide for districts which cannot now afford adequate educational programs through provisions such as those proposed by the Murray-Metcalf bill.

RATIONAL THOUGHT AND ANALYSIS

Reference has already been made to the new courses, written by teams of scientists and teachers, for a high school year of physics, chemistry, and biology; in mathematics for grades 7 through 12; and supplementary materials for the earth sciences. The mathematicians have wisely moved to a consideration of the elementary school, and next year a large number of elementary teachers will be trying out the new materials. A roadblock seems to exist in extending the efforts in the other sciences to the elementary and junior high school levels. There is not now a traditional or conventional science sequence for grades K through 9. A second even more difficult question is who should do this work and how should it be done. This work, obviously, cannot be done by a group of physicists or a group of biologists. On the other hand, one cannot make the assumption in starting the work that the program for grades K through 9 is necessarily to be general science. The woefully weak science program, K through 9, now in existence, owes its weakness in considerable part to the attempt to select a little bit from each of the several sciences and write it into one course, with resulting lack of continuity in the total program and a depressing amount of repetition. It is my recommendation, first of all, that the groups presently working in science explore what their science has to contribute at the elementary level, and at some later date, perhaps a year from now, that these materials be brought together in a ten-year unified sequence. It is not at all clear that this should be a sequence in general science. It may very well be that the best answer will come, for example, with the offering of biological science possibly in grades 5 and 6, and physical science in grades 7 and 8. Better still may be the teaching of botany in grade 4, astronomy in grade 6, and so on. It has by no means been proved that at the college level better general education in the sciences is to be had through integrated courses rather than through courses in the separate sciences.

This particular dilemma in science education is perhaps even best illustrated by mathematics. In some of the exciting new materials being tried out in the secondary school, there is great emphasis on structure and abstraction in mathematics, with less attention to applications. Very considerable evidence is available that these new approaches are accepted with genuine enthusiasm by pupils, teachers, and parents. In a recent news release from the University of Colorado News Bureau one reads:

> Boulder, Colo.—Imagine a whole class of seventh graders "lighting up like Christmas trees" in enthusiasm when asked how they like their mathematics course. Sound improbable? It happens again and again—junior high pupils declare their "math" study is "interesting . . . challenging . . . exciting . . . 'really neat.'" All this enthusiasm, in 30 classes in Colorado, and hundreds more in 11 other areas in the United States, is generated by new seventh and eighth grade mathematics material. It is the work of the nation-wide School Mathematics Study Group (SMSG) sponsored by the National Science Foundation.

A father of one now-excited seventh grader says: "This seventh grade mathematics has made a serious student of my daughter for the first time—it's the most stimulating study experience she's ever had!" and a teacher declares: "I enjoy teaching more than ever before—just a look at those 23 interested faces! Not a dull one in the class!"

The major critics of the experimental mathematics programs are those who believe that there should be greater early attention to important applications in the physical and social sciences, even though, for example, the seventh grade material does have a unit on the lever and one on probability. This criticism is clearly related to the broader question about integrated courses, in which traditional subject matter divisions disappear, an appealing concept but one which has not always been successfully applied. One of our most respected physicists is quoted as saying after reviewing the School Mathematics Study Group program that he hopes the mathematicians will soon develop a course in calculus for the secondary school. This hope will soon be fulfilled. In his look at the junior high school materials, he observed that the unit on finite mathematical systems, the most abstract part of the course, is the most valuable part for the future scientist and indeed for the future citizen. Is this observation the herald of a new spirit for science education in which "students will (truly) respond to an intellectual presentation of subject matter, in which rational thought and analysis" are of first importance and a primary goal?

CERTAIN AMBIGUITIES IN THE RELATIONSHIP BETWEEN SCHOOL, COLLEGE, AND UNIVERSITY

JAMES BRYANT CONANT

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(*Read April 22, 1960, in the Symposium on Current Issues and Readjustments in American Education*)

ANYONE who has tried to explain American education to a European audience must be aware of the difficulties in the undertaking. During my four years in Germany, I tried from time to time to tell German audiences about our schools, colleges, and universities. I always found that it was hard to find the exact words to convey to a German audience what Americans have in mind when they talk about their educational system. For example, the multitude of local school boards with their high degree of independence and authority completely baffles a German. "This is not a system," he is likely to declare, "but a chaos." To this I always replied that perhaps it was a chaos, but Americans seemed to like it, and I ventured to predict that the pattern was as stable as any aspect of our governmental structure. The comprehensive high school I, likewise, found difficult to explain. Indeed, the scepticism expressed by more than one German educator about the claims made on behalf of this characteristic American institution was one of the factors leading me to my recent study of the widely comprehensive high school. Almost equally confusing was the American idea of a college. I often tried in vain to portray the characteristics of this unique American institution. As a first approximation, one could say it represented something in between a German Gymnasium and a university. But when pressed for details, my explanation soon became too involved to be useful.

Germans are not the only people who are confused about American colleges. I remember a distinguished American official who was staying at the Ambassador's residence in Bonn. He asked me the name of the best German liberal arts college. When I told him there was no such thing as a four-year undergraduate college on the European Continent, I think he thought I was joking, but, as you are all aware, I was of course quite correct. The comparison which is often made between the large portion of our youth attending a college or university and the small portion enrolled in European universities is quite misleading. As all the members of this audience are well aware, a European university is essentially a collection of professional schools. If one compares the percentage of an age group attending universities in a European nation and attending our post-graduate institutions, the difference is not very striking.

The road to the university in Europe does not pass through the equivalent of an American college. Rather there are essentially pre-university schools for those who aim at a university education. They offer a stiff academic course of eight or nine years length centered on either (*a*) Latin and Greek or (*b*) Latin and a modern language (or two) and (*c*) modern languages, science, and mathematics. In each of the German states and in each of the Swiss cantons, a number of which I visited three years ago, one finds these three types of pre-university schools, each preparing students to pass examinations for a state certificate which, in turn, provides admission to a university anywhere in the nation. The difficulty of choosing students at the age of eleven or twelve for entry into one of these three types of pre-university schools is illustrated by a story I like to tell of the community in which some parents had objected because their children were not among those admitted to the pre-university school. The authorities had decided to try to cure the situation by having a parents' meeting each year and explaining to those who had children who were potential applicants for the pre-university schools what were the realities of the situation. They pointed out that the pre-university schools were extremely difficult and suitable for only the very bright; that one-half to two-thirds failed during the nine-year course; and that those who dropped out often suffered psychological damage. In short, they warned the parents that unless they were certain their child was very bright they should not attempt to have him enrolled in one of these schools. And, as a clinching argument, they went on to say "these schools, after all only prepare for the universities.

and the universities only prepare for the professions, and everyone knows there is no money to be made today in the professions!"

All of you who are familiar with American educational history are aware that the American liberal arts college is in the nature of an accident. During the nineteenth century the programs which were offered for the first degree in arts or in science or in letters were expanded to include a variety of practical subjects. This fact alone makes it difficult to explain to a European what is an American college. Earl McGrath's recent study has shown how wide are the offerings today, even in some of the older four-year liberal arts colleges. And we all know that, within a university in many instances, there is a great variety of four-year programs of study which may be entered directly by high school graduates.

At first one is tempted to say that an American college is intermediate between a high school and a "university" in the European sense of the word. It is true, of course, that for the most part those who become lawyers now study three or four years in an undergraduate department of a university or in a four-year liberal arts college before they enroll in a law school. The same is true for medical students. On the other hand, because of an historical accident, the engineering schools never became post-graduate schools. As a consequence, high school graduates enter directly on their professional training if they want to become engineers. I might note in passing that this has certain unfortunate consequences in these days when we are anxious to draw into pure science as many able young men with a scientific bent as possible. A youth who starts to become an engineer is likely to end by being one since the profession is both attractive and lucrative. Law and medicine, on the other hand, recruit from college students who have an opportunity during three or four years of college to consider what should be their future profession. This is true also for those who are going to work for the Ph.D. degree in physics, chemistry, biology, and mathematics. In these subjects, as in the social sciences and the humanities, professional training, so to speak, may start as early as the sophomore college year. With all these complications in mind, it is hard to define without ambiguity the relation of a college to the education of the members of the learned professions.

There is still another ambiguity inherent in the American pattern of higher education. The standards for admission to the American colleges and universities are so varied as to make it impossible to describe them. Indeed, I think few Americans realize how wide is the range of tolerance. I have talked to guidance officers in certain suburban high schools which serve a community where almost all the parents demand that their children enter some sort of college. This demand creates problems for the school in the case of some youth with little academic talent. However, experienced guidance officers in this type of suburban high school have learned a great deal about the admission requirements of the nearly 2,000 institutions of higher learning. I was assured by one such man not long ago that he could find some college which would admit any boy who had an I.Q. above 90 and who would do a little work!

Standards for the first degree are almost as diverse as the standards for admission. The officers in charge of admission to graduate schools of law, medicine, business, and arts and sciences are well aware of this fact. They know what many Americans also know—namely, that the mere holding of a bachelor's degree has almost no significance. Yet no one would publish a rank list of institutions or even be willing to be quoted publicly to the effect that a straight "A" record in one college has about the same meaning as a "C−" record in another. In the United States, we have a whole hierarchy of institutions granting the first degree and a complete conspiracy of silence as to the existence of such a hierarchy. This is a consequence of the American doctrine of equality of status, and I do not decry it. It does render ridiculous, however, any talk of raising entrance requirements the country over in order to improve the work of the high schools.

There has been a great deal of talk about raising standards in the last few years and more than one reference to Europe, where a state examination determines whether a youth is considered well enough educated to enter a university. There has never been any comparable examination in the United States, and there probably never will be; nevertheless, I venture to suggest that leading professional schools of law and medicine and graduate schools of arts and sciences might require their candidates to pass a set of examinations which would demonstrate a mastery of academic subjects somewhat comparable to that required for entrance into a European university. The requirements might be as follows: a good

reading, writing, and speaking knowledge of at least one modern foreign language; a knowledge of mathematics through the calculus; a knowledge of physics, chemistry, and biology at the freshman college level; the ability to write a competent essay; a knowledge of American history and political institutions at a sophisticated level; and a knowledge of English and American literature at a corresponding level.

The variety of our colleges prevents any uniform set of standards for admission. Therefore, the colleges could not, even if they desired, greatly influence the high school curricula. But the relatively homogeneous nature of the graduate and professional schools makes the formulation of standards possible. The effect of this formulation would influence both colleges and high schools. I should point out that if action such as I have suggested were to be taken by the American Association of Universities, for example, a warning period of at least five years would be required. Given ample warning, colleges and high schools could then adjust their programs. Whether or not there be any merit in my radical proposal, I hope that in this meeting of the American Philosophical Society dedicated to "The Current Issues and Readjustments in American Education" there might be a consideration of my basic assumption—namely, that professional men and women should have a wide as well as solid academic education. Unless the leaders of the bar, of medicine, and the professors of the academic subjects are willing to agree to this assumption and are ready to attempt to implement it, I see very little use in talking about raising standards in terms of the mastery of subject matter at the secondary school level.

THE GOALS OF HIGHER EDUCATION

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(*Read April 22, 1960, in the Symposium on Current Issues and Readjustments in American Education*)

IN THE broadest sense, the goals of higher ducation are identical with those of all educa- on: the development of an informed, respon- ible citizenry and the preparation of every boy nd girl for a personally satisfying and socially seful career. If we are to achieve these goals, we are to capitalize to the maximum the intel- ectual resources of our youth, it is time for us to ake a fresh look at our present educational tructure and philosophy.

Under the pressure of numbers and to facilitate dministration we have developed in the last entury a rigid institutional hierarchy, determined argely by age and divided into segments of a ixed number of years. And each of these units as been organized as an autonomous institution vith little regard for the other units. We have ome to think of education as made up of a series f discrete layers.

Now this structure has little relation to the ndividual student. First of all, the intellectual levelopment of the individual is an unbroken, ontinuing process. And secondly, the individual lifferences in aptitude and capacity for intellectual naturation are so great that no single order of rogress is possible. We must, then, start with a new concept of the unity of the educational rocess, from kindergarten to professional school, in which each stage is a preparation for the en- suing stage, whether in the workaday world or in further education. And we must provide a maximum flexibility so that every boy and girl may proceed as far and as fast as his ability and interest permit.

It would be presumptuous of me to attempt to prescribe the content of instruction at each of the levels. But there are certain general principles that seem to me clear. At every stage there must be a common core of basic skills, information and experience appropriate to the intellectual level of the student. Our standards of performance must be high; we must not underestimate the student's capacity for learning. And above all we must demand that every boy and girl be ready to work hard, to give the best of which he is capable.

It may well be that this will mean the elimination of some subjects from the curriculum and their relegation to the area of extra-curricular activity, for if our schools, particularly the secondary schools, are to provide the necessary preparation for advanced study, they must insist upon solid training in the basic studies, such as mathematics, foreign languages, history, and science, as well as in English.

On the basis of the student's performance in these essential studies and of such tests as are available we must try to identify the particular career for which he is suited and especially to discover the few who by their talent give promise of intellectual leadership. If it appears that a boy or girl has no aptitude for book-learning, it is folly to require him or her to idle away years in school in a state of utter boredom. Our laws that forbid teen-agers to work gainfully may be a contributory cause of juvenile delinquency.

The same principle applies to college education. We must get away from the notion of college as four years of marking time before a boy starts on the business of life; we must discard the nostalgic slogans of "bright college years" and "the stein on the table." College education is not a right; it is a privilege available only to those who merit the opportunity. In this connection it should be remarked that colleges must be increasingly discriminating in the selection of their students. Most of the private institutions have already moved in this direction. But the state institutions, as well, must adopt this policy. It is financially wasteful and personally frustrating for a state university to admit every high-school graduate, only to drop a quarter of them by the end of the first year. If we could eliminate from college both those who are unfit and those who are unwilling to give their serious best to their studies, it would make room for at least a part of the throng of deserving students who tomorrow will be knocking at the doors.

In general, then, at all levels we must transfer our center of interest from the institution to the individual student and his intellectual growth. If a boy or girl can master the subjects of the grade

school in five or six years, he should not be held to eight years of servitude. There is nothing sacrosanct about a four-year high school or college. Many a boy, if encouraged, could be ready for college after three years of study in a secondary school; many a college student could enter a graduate or professional school with less than four years of preparation. Let us give up the system of measuring progress by credits or points or courses and base our judgment on achievement. Only thus can we provide the incentives which will promote the early development of a new generation of potential leaders.

Within this general framework, let us look at the particular problem of higher education. If, as I assume, higher education refers to all institutions above the level of the secondary school, we are faced with a wide variety of types: junior, or community colleges, technical schools, schools of art and music, liberal arts colleges, undergraduate professional schools, graduate schools of arts and sciences, graduate professional schools, even "adult education." I cannot hope to examine each of them in detail, but there are certain tasks that all of them are called upon to perform.

All types of higher education have a dual function: the formation of a liberally educated public and the training of the individual in a special field of competence. Let me begin with the junior college, which I believe is destined to play an increasingly important role in American life, since it provides an educational opportunity for a large number of students who, living at home or within commuting distance, can better prepare themselves for the future. The junior college must provide two types of curriculum: one for those who will enter the working force at the end of their two years of study; the other for those who will continue their education at a liberal arts college or professional school. Both groups will need a common experience of liberal studies, but the college-bound group must also have the basic courses that are prerequisite for advanced studies and the other group should have the essential preparation for a career in some field of work as technicians or semi-professional workers. I do not mean for a particular, limited field, but rather an experience in fundamental techniques, such as mathematics, draughting, or design, which may be valuable in a variety of fields. But again, I am not competent to describe the details.

The most serious problem of the junior college is that of staff. It will not suffice merely to transfer high-school teachers to the junior-college faculty. That would mean adding two more years of secondary instruction to the present program. Particularly for those students who plan to continue their studies, it is essential that their instructors possess a depth of training comparable to that of teachers in a liberal arts college, otherwise they would find themselves unprepared for advanced work. I do not have a solution to propose, but I would call your attention to the tendency everywhere for the junior colleges to expand into four-year colleges. If that should happen generally, it would be possible for them to attract an increasing number of adequately trained teachers, who at present shun the junior colleges.

Let us turn now to the largest and most critical segment of higher education—the liberal arts college. First of all its relation to the secondary school. We are all aware of the strange anomaly of our present situation. Our colleges are teaching students elementary skills which presumably are the concern of high schools: English spelling and punctuation, elementary foreign languages or mathematics. They are repeating subjects—shall we say American history—to which the student has already been exposed two or three times. Perhaps I should not mention science, out of regard for my distinguished colleagues on this program, but it has always puzzled me that professors of chemistry, for example, give no recognition to the prior study of that science in secondary school but start their students anew, in fact sometimes feel that high-school chemistry is dangerous, as imparting bad habits and distorted notions.

The picture is not wholly black. In recent years, by closer cooperation between the colleges and the schools, the number of students admitted with advanced standing has been increasing. There have been promising experiments in the admission of superior students before their completion of a prescribed period of four years. But we have made only a beginning. More and more we must define what it is possible for the schools to achieve, we must establish minimum standards of performance, must build our college program on the foundation of prior study.

Like other institutions, the program of the liberal arts college has two goals: the preparation of a liberally educated public and the training of the individual student in a specialized field, either as a preparation for graduate or professional study or as an experience in depth at a mature intellectual level. In some colleges the

program designed to attain these goals is divided into two sections: two years of elementary "general education" followed by two years of concentration in a special field. Such a division is unfortunate; it leads to the notion that liberal education is something to be completed, put behind. Ideally, at all levels the two goals should be kept in mind from the beginning to the end and liberal studies, at an increasingly mature level, should remain a continuing element in the curriculum.

Another important function of the college is the early identification and encouragement of the superior student. Honors programs for upperclassmen have long been familiar. But in recent years a number of colleges have been experimenting with honors programs beginning in the freshman year. The experiment is promising, for it gives to the talented student an opportunity to develop his own capacity, freed from the mechanical trammels of rigid requirements; and, what is more important, it places on him the responsibility for educating himself. It is time for us to get rid of the idea that the only way a student can learn is by listening to a teacher.

Beside the typical liberal arts college there is still a large number of undergraduate professional schools. It is my belief that their days are numbered, for it is clear that it is not possible, in a four-year course, for the average student to acquire both a liberal education and adequate professional training. Years ago the schools of law and medicine, which once were undergraduate schools, found it necessary to require a wider pre-professional experience. And this tendency is already extending to other professional schools such as business administration and social service.

It would be unwise to require a bachelor's degree for admission to these professional schools. The programs in business administration, education, engineering and the like should be organized in connection with the program in liberal arts. Together they will require, for most students, five or six years of study and will be recognized by a degree of bachelor of arts and by a master's degree in the professional field. As in the college, a division into separate periods of two or three years, the first part devoted to liberal studies, the second to professional training, is undesirable. The first years will naturally be more concerned with breadth of intellectual experience, but there will be room for the basic studies prerequisite to advanced professional work and the last years will become increasingly specialized. But to the end of the course there will be a place for broadly oriented liberal studies.

Our present system encourages the production of a half-trained, illiterate generation of engineers and teachers. In the field of professional education we have witnessed a remarkable development in our time: "normal schools" became "teachers colleges"; now they are becoming "state colleges." But the development will be meaningless if it involves merely a change in name. These new "state colleges" must become liberal arts colleges. If they do, there will not be time enough in their program to permit the adequate training, in a four-year course, of the teachers we need for our schools. They must therefore design five- or six-year programs of the type that I have just mentioned. There is no way of improving the quality of primary and secondary education except by raising the standards of our teachers. Technical or professional competence is not enough. We need teachers whose cultural background is sufficiently broad and deep to enable them to serve at once as examples and as inspiration for our boys and girls.

The practice of combining liberal and professional studies which I have discussed as desirable in undergraduate schools should also be extended to graduate schools, including the graduate school of arts and sciences. Schools of law and medicine should not remain isolated from the basic disciplines but could profit greatly by the inclusion of related liberal studies in their programs. Some steps have been taken in this direction. At Michigan the Law School faculty a few years ago designed a joint program in conjunction with the College which included basic studies throughout the whole six or seven years. And the experimental program in medical studies at the Johns Hopkins University is a move in the same direction.

This broadening of the base is particularly necessary in the programs of the graduate schools leading to the Ph.D. degree. The greatest danger in graduate study is the increasing narrowness of the training; we are by way of producing technicians, not scientists; fact-collectors, not scholars. To meet the situation adequately we should need a new philosophy of the purpose of graduate study. As a minimum I would propose that every doctoral candidate be required to demonstrate by examination his familiarity with at least one other field than that of his major interest.

By now it should be evident what I consider to be the most important task of higher education: the integration of liberal and specialized studies in a single unit of learning. This integration is essential at all levels; it is the basis of coordination of all of its parts. If it is to be effective, it must produce a new generation of teachers. We cannot afford to consider the training of teachers as the exclusive concern of departments or schools of education; the subject matter departments have an equally important role to play. In a word, we must return to the older concept that the training of teachers is a major responsibility of the whole college or university. And I mean teachers at all levels—elementary, secondary, and college.

It is hard to explain the indifference of most of our academic scientists and scholars to the problem. They seem to forget that the quality of their own graduate students will depend upon the kind of teaching they have received in secondary school and college. It is disconcerting to hear professors of mathematics declare that they have no concern with the training of teachers at the very moment when the president of their institution is berating the secondary schools for their failure to provide adequate instruction in mathematics. For my part, I am convinced that our graduate schools must accept the responsibility of training teachers, especially secondary school and college teachers. I will not review here the old question of the appropriate degree to be granted, though I must say that I am inclined to the belief that the degree of "Doctor of Philosophy" should be restricted to those who are going to teach; they are the ones who must, by virtue of their function, be "philosophers." The others, who will enter on a career of research, would more appropriately be called "Doctors of Science." But whatever the degree that is granted, what is most needed is a new attitude toward the importance, the dignity, the necessity of teaching. The present concentration on research as the only function of the graduate school will in the end destroy the very foundations of a liberally educated society.

Before I close, there is one other problem to which I must devote a word: the question of financial support of higher education. For if we are to attain the goals that I have indicated, two things are necessary: the development of a student body capable of intellectual growth and the recruitment of faculties of superior quality. After the last war, our government made provision, as a token of gratitude, for the further education of the men who had given their service to the nation. I cannot escape the feeling that today, as we face the national and international problems of the years ahead, it is even more imperative, as a matter of national interest, that every boy or girl who has the ability should be assured of an education up to the limit of his or her capacity. Certainly the exceptional few who give promise of social or intellectual leadership should not be lost to society for lack of financial means. Some of them can be taken care of under our present system of scholarships and loans. But as the number of potential candidates increases I see no alternative to some form of federal support of this élite. It will cost money, a lot of money, but it may well be that a major investment in our youth will prove a better measure of national defense than a few more rockets destined to fizzle on the launching pad.

The recruitment of superior teachers is equally important and there is no easy solution of the problem. All the evidence points to the fact that the teaching profession is not attracting the best minds. This is not surprising, for a career in teaching offers neither the financial rewards nor the social prestige of other occupations. In the last few years there have been a number of efforts, private and public, to induce superior students to prepare for teaching by the provision of fellowships. But they will little profit, if the student finds that as a teacher he cannot hope to support a family with the decency and dignity which the other professions and business afford. It is true that in recent years a few institutions have made substantial increases in professorial salaries, to the point where they are almost competitive. But the rank and file of teachers are still receiving salaries that are a disgrace to the nation. I would not venture to suggest that professors should hope to compete financially with doctors, unless they can develop an aggressive union of the type of the American Medical Association. But should they not earn at least as much as plumbers or "disk jockeys"? Money is not the only thing that will appeal to the better minds, but it is a *sine qua non*.

In many European countries teachers are poorly paid, but at least they enjoy a prestige in the community which compensates for their underprivileged economic status. Here, however, the teacher has no such rewards. In spite of fair words, the popular stereotype of the college professor is still one of befuddled, if harmless futility.

I fear that that image will persist. A few years ago one of my students remarked that if the College continued to raise its admission standards and failed to raise its professors' salaries, the students would soon be intellectually superior to their teachers. Beneath his "wise crack" there was a solid kernel of truth. I have no panacea to offer as a remedy. I suspect that we are faced with a basic problem of contemporary American culture: the low repute of non-material values. And cultural traits are not easily changed.

I would not end on a note of pessimism. Within our time we have made extraordinary gains in education; for the first time in history we have made schooling possible for every boy and girl. In the effort to achieve this goal we have, no doubt, been forced to accept an ideal of mediocrity; we have failed to provide for the superior few the opportunity and the encouragement which they must have if we are to develop the leaders of the future. Our educational structure must facilitate this process. It must be more flexible; it must be coordinated in a continuing, integrated program. Only thus may we hope to attain the goals, not only of higher education, but of all education.

THE AMERICAN COLLEGE IN DOUBLE JEOPARDY

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(Abstract of remarks delivered April 22, 1960, in the Symposium on Current Issues and Readjustments in American Education)

DISSATISFACTION and agitation for the reform of our school system antedates Sputnik and has deeper roots than the desire to compete in the production of engineers and scientists. The movement toward General Education, the scepticism about the methods for teaching the very young how to read and write, and that impatience with the high school which led to the Advance Placement system are the significant moments in an evolution dating back to the end of the Second World War.

The most recent results are familiar: the high schools have begun to reform their curriculum and to provide for acceleration: everybody connected with or affected by education lives in perpetual apprehension about "standards," and the debate is joined once again on the perennial question, What Shall We Teach?

The college has not undergone as much criticism as other parts of the system, but disquiet is present there too, and changes in attitude are clearly marked. When I say that the college is in double jeopardy, I have in mind the two forces that seem to me to be destroying the very idea of college education and leaving nothing defensible in its place.

The first of these forces is the upward thrust into the freshman class of a large number of very able youths who are full of curiosity, conscientious, and quite capable of hard work, but who have not been taught the fundamentals. They have on the contrary learned to regard attention to detail as the antithesis of intelligence. To them and their teachers, the signs of high ability are originality, fertility in opinion, imagination (plausible or not), and the willingness to differ, which is called independence of mind. These ideas are indeed tempting and no one would dispute them in the abstract, but they produce only caricatures of the virtues desired when they are not grounded in the command of fact and the ability to learn and to reason.

In practice, the best young men and women in our colleges tend to be the least instructed, the most undisciplined and "viewy." They are correspondingly hard to teach, and the complaint of teachers who analyze the difficulty is that they must impart the rudiments of reading, writing, counting, and reasoning while also trying to teach new subject matter in the established disciplines. The steady dissatisfaction with the Freshman English course, the no less continual recasting of the main introductory courses, the rooted despair of the language departments are so many witnesses to the earlier failure of our preparatory system, even when it is most desirous to foster native ability, reduce waste of time in the eleventh and twelfth grades, and turn young minds loose on serious subjects.

So much for the first destructive push against the college. It comes from below and has the effect of dividing attention and effort at a time when new disciplines and new facts should be assimilated for their intrinsic and forward-looking interest, instead of being used as means of diagnosis for earlier defects and as materials for remedying those defects.

The second disturbing element arises within the college faculty and among the best students in the form of an obsession with "standards." It is a natural reaction to the manifest looseness of previous work; it is an attempt to give at long last to the sophomore or junior the intellectual command of *something*. The desire expresses itself in a number of forms: it may be a requirement to take a course or sequence of courses having a quasi-professional character—the student of economics *shall* learn that subject as if he were to become a scholar in economics, a practicing professional economist; the student of English literature must subject himself to a course of textual and scholarly criticism as if he were going to edit Spenser's *Faerie Queene*.

Or again, the requirement is for concentration on a major subject. The number of hours devoted to that one subject is supposed to insure mastery of it and this is deemed preferable to a division of interest and a more exploratory attitude. Much use is made also of honors programs in which so-called independent work and

genuine research are required—I say required, for these are not always forthcoming by themselves, or if they are it may be in the form of poor imitations. This should not be wondered at, since little or no preparation and instruction in that kind of work has been given.

The emotions that go with these efforts are in part laudable and in part suspect. Some faculty members themselves derive kudos from being in charge of honors or from being chosen by students as sponsors of honors work. The students also regard the work as superior in kind over "mere learning." They are anticipating the highest academic activity they can think of, which is to do research. It is no matter that the attempt is ill-grounded, sometimes ill-directed, and in most cases repetitive or trivial. The type of performance dignifies the whole and makes acceptable the uneven distribution of time and effort, the neglect of whole ranges of common knowledge.

If this fairly describes a double tendency now at work in our very best colleges, what are we to say about it? It rests with the local authorities in each case to do what they want, but we are perhaps permitted to observe and to report impressions that are easily duplicated and verified.

We must say, then, that for the student with normal or marked intelligence the college runs the risk of becoming a mere wayside inn. Here he stays in no settled course, but part of the time is catching up with his deficiencies, part of the time groping among new subjects, part of the time playing at the role of scholar and researcher. The level of his performance is nowhere fixed by his years or his progress, nor is the body of knowledge to which he is exposed in any sense coherent. In other words, the necessary function that must be fulfilled in any system of higher education, the *gymnastic function* (the teaching of the *gymnasium*) is nowhere carried out. The high school only stimulates, the college half remedies and half accelerates, which leaves the graduate schools to make professionals out of ill-organized, one-sided, minds, highly intelligent and trained to competence, but in no sense literate, well informed, or educated.

ON THE DOCTRINE OF ORIGINAL ANTIGENIC SIN *

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(*Read April 21, 1960*)

INFLUENZA has always been a mixture of romance and terror; of fact and fable; of new and old ideas. Its various popular names: the jolly rant, the new delight, the newe acquayantance, gallants' disease, the fashionable illness, influenza di freddo or influenza di coeli, la grippe, flu, this virus thing—all indicate a light-hearted annoyance. But interspersed are the tales of damaging experiences. The sweating sickness of England of 1485 which decimated Richmond's Army after the defeat of Richard III at Bosworth Field is thought by some to have been influenza. The pandemic of 1743 appears to have been calamitous. The term, blue plague, accredited to Horace Walpole, or the name Blitzkatarrh forecast features of the devastating episode of 1918.

The historical disease, nevertheless, conforms in its epidemiological and clinical characteristics to what we now call influenza. That disease in its sporadic, epidemic, or pandemic form is caused by one of the influenza viruses. The influenza viruses comprise four types: A, B, C, D. The Type A viruses with which we shall here be concerned, comprise, in turn, four groups or families whose members or strains may vary in some characteristics, but retain a close group relationship (fig. 1). (The numbering of these families is inconsistent, because it is difficult at present to decide what order to assign to them.)

Influenza virus is considered to consist of a core of RNA which is much the same in all strains of the type. Surrounding the core (fig. 2), toward or at the surface of the virus particle is an accumulation of mucoprotein components which possess group and strain characteristics;[1] they also carry the antigenic properties which stimulate immunity and specific antibody response after infection or vaccination. It is these immunizing antigens which will be primarily discussed in this presentation.

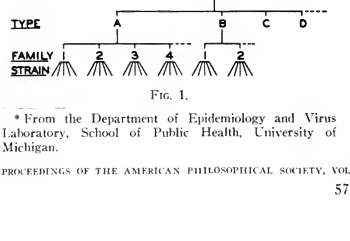

FIG. 1.

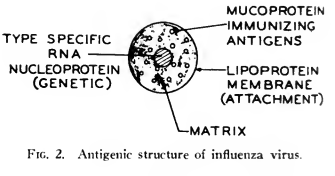

FIG. 2. Antigenic structure of influenza virus.

It should be understood that an antigen is a substance which induces the formation of antibodies, and that an antibody is serum protein whose production is stimulated by antigen. It combines specifically with the corresponding antigen. Antibody is the major factor in acquired immunity.

Our concept of an influenza virus belonging to Type A is that it contains a series of immunizing antigens shared by strains of that type (fig. 3). Within a family one or perhaps a set of these antigens is the dominant. Others occupy secondary or lesser status whose presence can, however, be demonstrated by the antibody response of ani-

* From the Department of Epidemiology and Virus Laboratory, School of Public Health, University of Michigan.

[1] Hoyle, L., The multiplication of complement-fixing antigen and red-cell agglutinin in the chorio-allantois membrane of fertile eggs inoculated with influenza virus. *Jour. Path. and Bact.* **64**: 419–423, 1952; Hoyle, L., and W. Frisch-Niggemeyer, The distintegration of influenza virus particles on entry into the host cell. Studies with virus labelled with radiophosphorus, *Jour. Hyg.* **53** 474–486, 1955; Frisch-Niggemeyer, W., and L. Hoyle, The nucleic acid and carbohydrate content of influenza virus A and of virus fractions produced by ether disintegration, *Jour. Hyg.* **54**: 201–212, 1956.

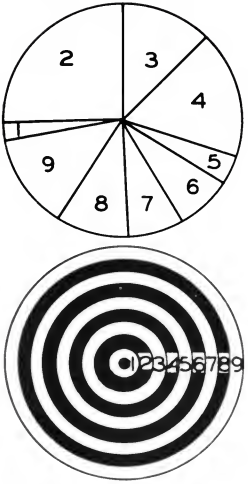

FIG. 3. Possible antigenic arrangement in strains of influenza virus.

mals to inoculations with one of the strains. As early as 1935, it was possible to show serological relationships between the newly isolated strains from human influenza and the swine influenza virus described by Shope.[2] Studies in 1938 of the differences between human strains also emphasized their common antigens.[3] In a later study the presence of 18 antigenic components was demonstrated and indications of several more

[2] Francis, T., Jr., and T. P. Magill, Immunological studies with the virus of influenza, *Jour. Exp. Med.* **62**: 505–516, 1935; Francis, T., Jr., and R. E. Shope, Neutralization tests with sera of convalescent or immunized animals and the viruses of swine and human influenza, *Jour. Exp. Med.* **63**: 645–653, 1936.

[3] Magill, T. P., and T. Francis, Jr., Antigenic differences in strains of epidemic influenza virus: I. Cross neutralization tests in mice, *Brit. Jour. Exp. Path.* **19**: 273–284, 1938. Francis, T., Jr., and T. P. Magill, Antigenic differences in strains of epidemic influenza virus. II. Cross-immunization tests in mice, *Brit. Jour. Exp. Path.* **19**: 284–293, 1938; Smith, W., and C. H. Andrewes, Serological races of influenza virus, *Brit. Jour. Exp. Path.* **19**: 293–314, 1938.

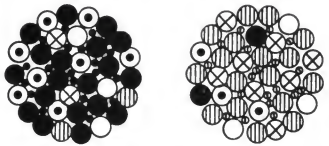

FIG. 4. Rearrangement of antigens in strain variation among influenza viruses.

were obtained.[4] Variation among the influenza viruses is then visualized as a rearrangement of the components quantitatively—or in their potential for antigenic expression—whereby the dominant antigen of one strain may be largely suppressed in another—or *vice versa* (fig. 4). Antigenic alteration can also be induced under a variety of experimental conditions, thus emphasizing the ready variability of this group of viruses.

Some investigators support another view, suggesting that with the passage of virus through the population it is progressively losing old antigenic components and gaining others not previously present (fig. 5).[5] But this is not in keeping with the evidence; the studies of Jensen and Peterson[6]

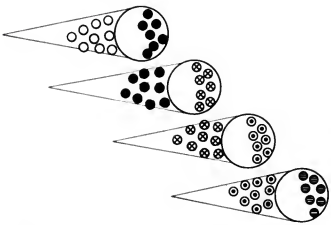

FIG. 5. The concept of antigenic loss and gain in variants of influenza virus.

[4] Jensen, K. E., and T. Francis, Jr., The antigenic composition of influenza virus measured by antibody-absorption, *Jour. Exp. Med.* **98**: 619–639, 1953.

[5] Andrewes, C. H., Adventures among viruses. II. Epidemic influenza, *New England Jour. Med.* **242**: 197–203, 1950; Factors in virus evolution. Advances in virus research **4**: 1–24, New York, Acad. Press, 1957.

[6] Jensen, K. E., and W. D. Peterson, Jr., Comparative measurements of antigenic differences among human and swine influenza viruses, *Jour. Immunol.* **78**: 365–372, 1957.

point out that the strains of a family do not vary in an orderly chronological progression during its period of prevalence but in a random manner with time. Moreover, strains recently in circulation may induce antibody to strains of twenty-five years ago under experimental conditions and under natural conditions as well, since an occasional child will present antibody to strains of much earlier distribution. The concept of a rearrangement of antigens under the pressure of population immunity, a rise and decline rather than a gain and loss, appears more fitted to the available evidence.

The epidemiologic significance of the demonstrable variations in influenza virus has largely been derived through study of the characteristics of strains recovered during numerous epidemic prevalences in conjunction with antibody measurements in patients or in the general population and in relation to immune responses after vaccination. In the United States there have been thirteen general epidemics of influenza A since 1934. The strains of virus isolated from 1933 to 1943 have been considered to be of the same A family (A-PR8), although numerous variants have been recognized. The classical studies of the Commission on Influenza in 1943 emphasized their comparative unity by showing that vaccine containing a 1934 strain gave excellent protection against the 1943 epidemic virus.[7]

In 1947 a new family appeared to which the younger segments of the general population, at least, had little strain specific antibody. Moreover, vaccination of such subjects with older strains elicited little or no response to the new strain. This family, which was called A-prime, continued in prevalence from 1947 to 1957.

It appears then that about ten years were required for virus of a family to circulate in the population so extensively as to build up immunity to the point that virus of that group did not find ready susceptibles in which to propagate. When this degree of resistance is reached, according to our concept a rearrangement of antigens may be expected in the virus, yielding a modified strain with a dominant antigen to which a large proportion of the population is not adequately immunized.

To obtain better information on the immunity of the population, sera were collected in 1952 from a random sample of 1,250 persons from infancy to old age. They were tested for antibody to a number of strains isolated from human subjects from 1933 to 1953. Swine influenza virus was also included. Three distinctive patterns of antibody were disclosed.[8]

1. The sera of children contained antibodies essentially oriented to the A-prime strains which had been current for five years. They were highest in the five to twelve year age group, but after fourteen to fifteen years of age the levels fell sharply (fig. 6).

2. Antibodies to the original A group were first detectable at eleven years with a peak among persons of seventeen to twenty years of age. The

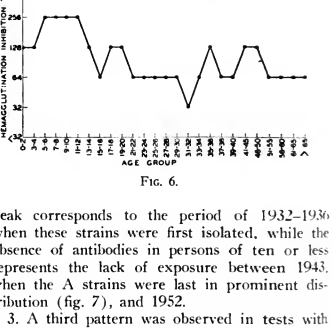

FIG. 6.

peak corresponds to the period of 1932–1936 when these strains were first isolated, while the absence of antibodies in persons of ten or less represents the lack of exposure between 1943, when the A strains were last in prominent distribution (fig. 7), and 1952.

3. A third pattern was observed in tests with swine virus. No antibody was detectable until twenty-nine years of age, suggesting that the dispersion of this major antigen among men ceased in the 1920's. The peak occurred at thirty-five to thirty-nine, representing persons who were born in 1914 to 1917 (fig. 8).

The first two patterns clearly show correspondence in time with the known periods of prevalence of the A and A-prime viruses. The inference, then, is that a virus similar antigenically to the

[7] Francis, T., Jr., (For summary see chapter on Influenza) *Viral and rickettsial infections of man*, ed. by Rivers and Horsfall, Third edition, Lippincott, Philadelphia, 1959.

[8] Davenport, F. M., A. V. Hennessy, and T. Francis, Jr., Epidemiologic and immunologic significance of age distribution of antibody to antigenic variants of influenza virus, *Jour. Exp. Med.* **98**: 641–656, 1953.

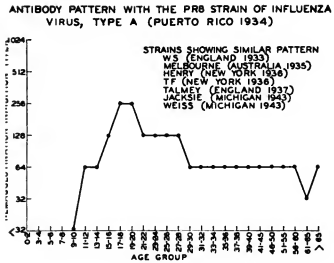

FIG. 7.

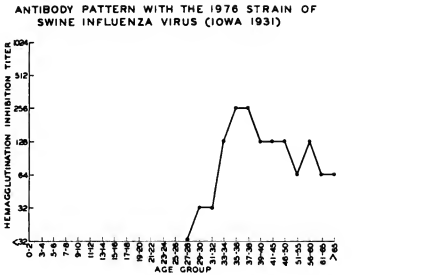

FIG. 8.

wine influenza virus was the agent prevalent in ian during the period embracing the 1918 pan-emic. This proposal had been made in 1935 y Laidlaw and supported in 1936 by Shope;[9] t that time we did not concur. That the patterns re not chance situations is demonstrated by tudies of sera from England, Japan, and Czecho-ovakia[10] which have yielded essentially the same ictures emphasizing the world-wide distribution f influenza strains.

There are, in addition, other data which pro-ide historical perspective. In 1935 detectable ntibody to the human A strains was dominant in ne one to five year age group;[11] in 1948 none vas found before seven years of age;[12] in 1952 ney were first measurable at eleven to twelve ears of age;[13] in 1957 they were first noted about fifteen years of age.[14] The peaks of titers also moved upward (fig. 9).

In 1935, testing the same sera we had used, Shope found no antibody to swine virus before eleven years of age;[15] in 1952, seventeen years later, we detected them first at twenty-nine years of age;[16] in 1957 evidence of further progression upward was obtained in that the peak of titers was about five years later.[17]

It is interesting, too, that the antibody peak to A-prime strains which in 1952 occurred at five to twelve years was in 1957 advanced to nine to sixteen years.[18] Moreover, in 1952 high titers were noted even in the first two years of life; whereas, in 1957 significant antibody was first detectable about five years of age.

The shift in onset and peak of the respective antibodies according to age coincides remarkably

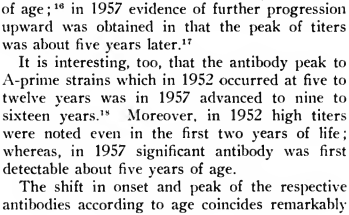

FIG. 9

[9] Shope, R. E., The incidence of neutralizing antibodies or swine influenza virus in the sera of human beings of ifferent ages, *Jour. Exp. Med.* **63**: 669–684, 1936.

[10] Davenport, F. M., A. V. Hennessy, C. H. Stuart-Harris, and T. Francis Jr., Epidemiology of influenza. Comparative serological observations in England and the United States, *Lancet* **2**: 469–473, 1955; Davenport, F. M., and A. V. Hennessy, The clinical epidemiology of asian influenza, *Annals of Int. Med.* **49**: 493–501, 1958; Blaskovic, D., and V. Rathova, Influenza virus A, A-prime, B, C, and Shope, Iowa 15 antibody titers in populations of Czechoslovakia, *Epidem., Mikro., Immunol.*, Czechoslovakia, **5**: 113–124, 1956.

[11] Francis, T., Jr., and T. P. Magill, The incidence of neutralizing antibodies for human influenza virus in the serum of human individuals of different ages, *Jour. Exp. Med.* **63**: 655–668, 1936.

[12] Melnick, J. L., and N. Ledinko, Social serology: Antibody levels in a normal young population during an epidemic of poliomyelitis, *Amer. Jour. Hyg.* **54**: 354–382, 951.

[13] Davenport, Hennessy, and Francis, 1953, *op. cit.*

[14] Davenport and Hennessy, 1958, *op. cit.*

[15] Shope, 1936, *op. cit.*

[16] Davenport, Hennessy, and Francis, 1953, *op. cit.*

[17] Davenport and Hennessy, 1958, *op. cit.*

[18] *Ibid.*

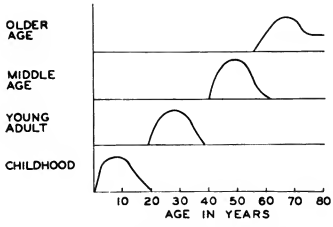

FIG. 10. The persistence of initial antibody throughout life.

with the passage of time. These two features in the age distribution of antibodies to the respective prototypes thus provide a serologic record from which the periods of prevalence of the different antigenic families of Type A virus can be reconstructed.

The antibody of childhood is largely a response to the dominant antigen of the virus causing the first Type A influenza infection of the lifetime. As the group grows older and subsequent infections take place, antibodies to additional families of virus are acquired. But the striking feature is that the antibody which is first established continues to characterize that cohort of the population throughout its life (fig. 10). The antibody-forming mechanisms have been highly conditioned by the first stimulus, so that later infections with strains of the same type successively enhance the original antibody to maintain it at the highest level at all times in that age group. The imprint established by the original virus infection governs the antibody response thereafter. This we have called the doctrine of original antigenic sin.[19]

The effect is attributed not merely to continuation of initial antibody levels but to repeated stimulation by persistence of the first dominant antigen as a lesser or secondary component of later Type A strains.

The interpretation is well supported by other experimental data. Ferrets have been infected sequentially with viruses of the different families with resultant antibody to all three. When either the second or third virus was mixed with the first serum, only part of the antibody was neutralized, but treatment of serum with the initial virus removed antibody to all three viruses.[20] Treatment of children's serum with A-prime virus removed antibody to all Type A strains; the prototype A strain, PR8, removed all antibody from the serum of young adults; swine virus removed all antibody from the serum of the middle aged. Clearly, the first experience has dominated specific antibody formation to related viruses encountered thereafter, but it also appears to control reactions to the total series of antigens present. Vaccination studies have further demonstrated the influence of primary education. Regardless of the strain of Type A used for vaccination, children responded most prominently with A-prime antibody, young adults with A antibody, and adults of 30 or more with antibody to swine virus.[21] The first infection thus governs antibody response to vaccination with other strains. This doctrine formulated with respect to the influenza viruses is becoming recognized as a more general phenomenon. Information from the field of typhus fevers, from the diffuse array of the encephalitic viruses and from study of chemically conjugated proteins is providing similar results.

It is apparent from the patterns of antibody presented that beyond the time of appearance and peak levels, antibody to each of the families discussed persist through the older population at a somewhat lower level. Part of this, at least, is believed to derive from the repeated influence of lesser antigens common to most strains. They create a composite immunity which tends to overlap strain and family specificities. Moreover, it seems probable that this broad antibody content and immunity tends to dampen the antibody response to dominant antigens of strains encountered in later years. It is an important aspect of our problem of strain variation and immunity.

RELATION OF IMMUNITY TO PANDEMIC RECURRENCES

Up to a point, at least, the immunity of a population represents a layering of the specific antibody pattern of one age group upon another. The incidence of influenza is always highest in

[19] Francis, T., Jr., The current status of the control of influenza, *Ann. Internal Med.* **43**: 534–538, 1955; Davenport, F. M., and A. V. Hennessy, A serologic recapitulation of past experiences with influenza A: antibody response to monovalent vaccine, *Jour. Exp. Med.* **104**: 85–97, 1956.

[20] Jensen, K. E., F. M. Davenport, A. V. Hennessy and T. Francis, Jr., Characterization of influenza antibodies by serum absorption, *Jour. Exp. Med.* **104**: 199–209, 1956.

[21] Davenport and Hennessy, 1956, *op. cit.*

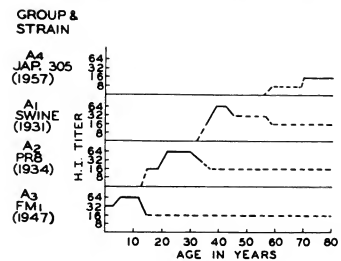

FIG. 11. Age distribution of antibody to different families of Type A virus (pre 1957 pandemic).

children, lowest in the old. The older segments have more layers of experience and of antibodies to multiple strains. But as they leave the scene, they are replaced by inexperienced infants. Their contribution to the immunity of the population is lost. When a virus group has thoroughly pervaded the population, its continuation becomes difficult because of the accumulated immunity (fig. 11). According to the hypothesis of rearrangement of antigens, the gap left in immunity by disappearance of immunological veterans and their replacement by inexperienced youth should provide an environment for establishment of a virus-variant with a dominant antigen not recently prevalent in epidemic spread. The result would be a variation forced by accumulated immunity toward the resurgence of older dominant antigens which could take advantage of the immunologic gap.

In 1955 we predicted the epidemic recurrence of a virus of this nature, suggesting it might be a swine-like virus—the oldest one of which we were informed.[22] But the 1957 pandemic of Asian influenza introduced a family of Type A virus with dominant antigen different from those of strains studied previously—apparently a new virus.[23] Mulder of Leiden, however, reported the recognition of antibody to the Asian virus in serum of persons of seventy years or older.[24] The observation was readily confirmed in this country, although in our laboratory the age appeared to be somewhat earlier, sixty to seventy or so, and occasionally antibody was detectable in the serum of a child.[25] Following the chronological interpretation of the doctrine, the indication is that the Asian prototype was not entirely new but had been in circulation about the period of the 1889-1890 pandemic. Our interpretation is that there was a recycling of antigenic structure resulting in a virus which gained entrance through the immunologically unguarded back door.

Mulder has proposed that the strain has been lying dormant in a hidden reservoir somewhere in China over this long period.[26] The suggestion supposes that the 1957 episode is then the recurrence of an intact strain of fixed virulence maintained unchanged over the interval of sixty years or more. I shall not discuss this concept in detail except to point out that the original sin of seventy to eighty years ago was in 1957 still imprinted on the youth of that era, that the antigen had not been lost, and that a cycle of recurrence such as we had postulated had been completed. The Asian family of viruses can now be expected to give rise to additional epidemics—as in 1960—for a period of several years, and then another dynasty will take over. But the Asian antibody will

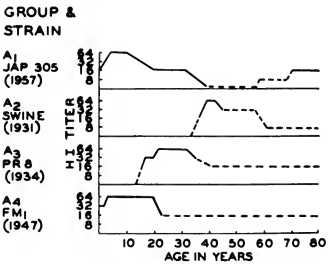

FIG. 12. Age distribution of antibody to different families of Type A virus (post 1957 pandemic).

[22] Hennessy, A. V., F. M. Davenport, and T. Francis, Jr., Studies of antibodies to strains of influenza virus in persons of different ages in sera collected in a post-epidemic period, *Jour. of Immunol.* **75**: 401-409, 1955.

[23] Meyer, H. M., Jr., M. R. Hilleman, M. L. Miesse, I. P. Crawford, and A. S. Bankhead, New antigenic variant in Far East influenza epidemic, 1957, *Proc. Soc. Exp. Biol. and Med.* **95**: 609-616, 1957.

[24] Mulder, J., Asiatic influenza in the Netherlands, *Lancet* **2**: 334, 1957; Mulder, J., and N. Masurel, Pre-epidemic antibody against 1957 strains of Asiatic influenza, *Lancet* **1**: 810-814, 1958.

[25] Davenport and Hennessy, 1956, *op. cit.*

[26] Mulder and Masurel, 1958, *op. cit.*

replace the A-prime as the dominant in our current young children and will typify them throughout life—just as the oldest segments were labelled in the nineteenth century (fig. 12). The A-prime antibody will move on just one stage ahead.

These observations provide historical information of the natural recurrences of influenza, and, in addition, serve as a guide to the promotion of useful knowledge as to what can be done about them. It has been adequately demonstrated that deficiencies in antibody of the different age groups can be filled in by vaccination with appropriate strains. It is our thesis that the variations in Type A influenza virus, for example, have a finite limit governed by the possible rearrangements of a number of antigens.[27] Vaccines have, therefore, been prepared which contain multiple strains representing dominant antigens of the various families as well as the secondary antigens which may cover a broader range of immunity. Children represent the most susceptible members of the population and probably the most important material for the building of epidemics. The gaps in their immunity should be eliminated by providing early in life the antigenic stimuli to meet the known or anticipated recurrent strains. Natural exposures would then serve to enhance the broad immunity laid down by vaccination. It is our hope that such vaccines can be made from pools of chemically purified antigens—or even with strains experimentally devised. In this manner the original sin of infection could be replaced by an initial blessing of induced immunity.

[27] Francis, T., Jr., Significance of antigenic variation of influenza viruses in relation to vaccination in man, *Fed. Proc.* **11**: 808–812, 1952.

LEVELS OF CELLULAR INTERACTION IN AMOEBOID POPULATIONS *

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(*Read in part, April 23, 1960*)

UNICELLULAR amoeboid forms of many kinds occupy a prominent place in the complex of microorganisms that inhabit soil and decaying vegetation. They vary in size from very minute, measuring only a few microns in diameter, to cells visible to the unaided eye. The majority of these amoebae lead a completely independent existence and increase in number by repeated division in the presence of adequate nutrient such as bacteria, yeasts, mold spores, and other microorganisms upon which they feed. Many of them have the capacity as individuals to form resistant cysts and so withstand protracted periods of nutritional and environmental adversity. Some have acquired a measure of social behavior reflected in their capacity to act collectively and to achieve in greater or lesser degree a multicellular organization during the process of fructification. It is of these social amoebae, assigned to the Class Acrasieae and commonly referred to as the cellular slime molds, that I shall speak today.

The cellular slime molds, as a group, exhibit three characteristics of special interest: (1) small myxamoebae that live independently throughout their vegetative stage aggregate to form cell communities (pseudoplasmodia) at the onset of the fruiting phase; (2) these cells progressively acquire functional specialization, and in the mature fructification (sorocarp) often show marked and divergent differentiation; (3) the walls of spores and elements of the upright stalk (sorophore) generally contain cellulose as a strengthening material. Thus in their life cycle, usually requiring from two to four days, these microorganisms progress from a free-living protozoan-like state virtually indistinguishable from true soil amoebae to multicellular plantlike structures of varying complexity that reflect the degree of cellular interdependence attained during cell aggregation and differentiation leading to sorocarp formation.

The Acrasieae do not, in all probability, constitute a class apart from the so-called free-living amoebae found in soil and aquatic habitats. Rather, they are believed to represent species and genera of such origin that have evolved through increased cellular association and interaction, coupled with a striking degree of cellular differentiation in the more advanced forms. In support of this thesis one can select a series of organisms which exhibits a gradual progression from free-living and consistently amoeboid types to species such as *Dictyostelium discoideum* Raper and *Polysphondylium pallidum* Olive characterized by symmetrical and beautifully proportioned fructifications with sharp demarcation between the encapsulated propagative cells that constitute the spore mass (sorus) and the sterile, pithlike cells that comprise the supporting stalk. Such a series is not regarded as a linear or monophyletic one since the cooperating amoeboid cells differ in the pattern of their nuclei and in the character of their pseudopodia. Additionally, the phenomenon of cell association, or aggregation, is not strictly correlated with that of terminal cellular differentiation, although the overall progression is certainly in that direction. Let us then examine a series of amoeboid organisms that have been carefully investigated in our laboratory and elsewhere and note the levels of cellular interaction and differentiation that are attained in each of them.

These may be considered conveniently in three categories, for each of which three genera will be reviewed and illustrated. The first group is represented by amoebae that behave essentially as single cells throughout their life cycle, yet possess limited capabilities for collective behavior, or for singular differentiation. The second embraces genera that exhibit true, if primitive, cell aggregation, but subsequent differentiation of the associated amoebae is generally insubstantial. The third includes those cellular slime molds wherein aggregation is dramatically expressed, and the assembled cells through progressive integration and dissimilar differentiation construct fructifications of unique and delicate patterns.

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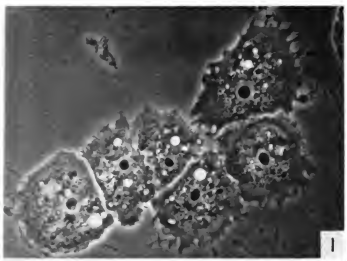
HARTMANNELLA

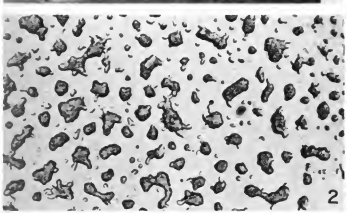
Passing over the many amoeboid forms which apparently exist only as independent trophozoites and show no tendency toward aggregation or obvious differentiation, we shall first consider *Hartmannella astronyxis* Ray and Hayes. This beautiful cyst-forming amoeba was isolated by Professor Dixie Lee Ray more than fifteen years ago, and what I shall say concerning it is taken in substantial part from her excellent reports (1951; Ray and Hayes, 1954). Applying the usual standards, *Hartmannella astronyxis* is in truth a free-living soil amoeba (fig. 1). Yet, at the time of encystment, which normally coincides with the exhaustion of the food supply (bacteria), the amoebae show a marked disposition to collect into irregular, platelike clusters of varying dimensions determined in large measure by the density of the population (figs. 2 and 3). It is not known whether this coming together of the amoebae represents a true chemotactic response or whether it is a chance convergence resulting from the continued movement and undirected contacts of an outlying population with a central and immobile cell, or cells, already undergoing encystment. One suspects the former, and Ray refers to the phenomenon as "aggregation." In any case, if only through cell adhesion, once an amoeba has come into contact with such a center it rarely moves away (fig. 4), but instead slowly ceases active pseudopodial movement, rounds up, and gradually transforms itself into a beautifully stellate cyst. It is quite possible that the maturing cysts secrete a substance which attracts neighboring amoebae, and having done this continue to hold them within the expanding cluster since it is the central cells which normally differentiate first; and in the completed plaques the star-shaped cysts lie in intimate contact, with the points of neighboring cysts often intermeshed like the cogs of adjacent wheels in a gear system (fig. 5). Such aggregation and possible intercellular control is not a requisite for cyst formation since isolated cells can and do form cysts indistinguishable from those formed in the plaques. The cysts within a plaque are normally disposed in a single layer and in no case is there any suggestion of a true fructification. Yet, in this species, we may be witnessing the beginnings of a phenomenon of cell aggregation; and terminal differentiation may be influenced in some measure by intercellular contacts and secretions. I am not proposing that *Hartmannella astronyxis* should or could be included in the Acrasieae, but I do suggest that it may exhibit a type of cellular behavior that is transitional between indisputably free-living amoebae and those that do aggregate and subsequently differentiate according to a specific pattern.

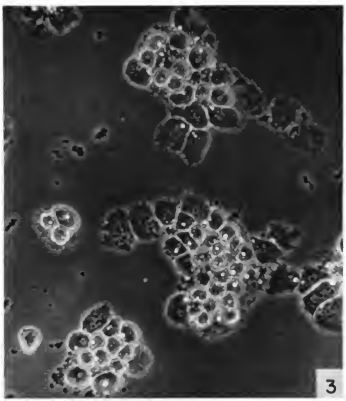
SAPPINIA

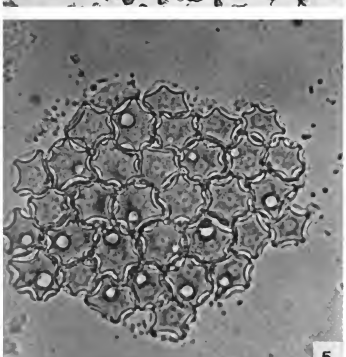
Traditionally, the genus *Sappinia* (Dangeard, 1896) has been regarded as the simplest representative of the Acrasieae. Following vegetative growth the amoebae may either (1) individually develop narrow pedicels and thus elevate their bodies above the substratum (fig. 9), or (2) collect into clusters at the end of raised projections where as more rounded cells they enter a transient resting stage. Upon the basis of this behavior, E. W. Olive included the genus, as represented by Dangeard's *S. pedata* (1896), in his Monograph of the Acrasieae (1902). This was done, however, with considerable misgivings. He noted that the binucleate amoebae with broad lobose pseudopods (fig. 6) were unusually large, and that in "germination" the so-called pseudopores, i.e. stalked or rounded individuals, returned to an active, vegetative state without casting off a wall of any kind. The aggregations formed at the distal ends of small projections from the substratum were thought to indicate a hydrophobic response of the amoebae at the time of fructification and not to result from a chemotactic stimulus such as he assumed must cause aggregation in genera such as *Dictyostelium* and *Polysphondylium*.

I am in general agreement with Olive's conclusions, but a few additional observations may be recorded. While basically independent and free-living, the amoebae of *Sappinia* show a marked tendency to collect into clusters of varying size under situations where differences in moisture can hardly be responsible (fig. 7). Additionally, some of these aggregates, if we may call them such, form during the vegetative phase and may but need not proceed to the resting stage. Some aggregates possess the capacity for unitary movement and the manner in which they clear the bacterial growth (fig. 8) provides evidence that even here the component cells are still actively vegetating. As a matter of fact, we know relatively little regarding the nutritional and environmental factors that favor fructification in *S. pedata* and other stalked amoebae that behave in a comparable manner. When first isolated from soil, and particularly in cultures with a mixed

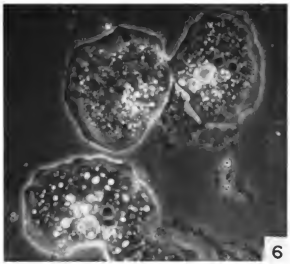


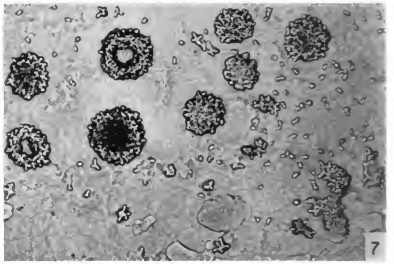


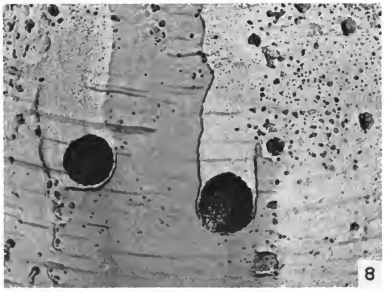


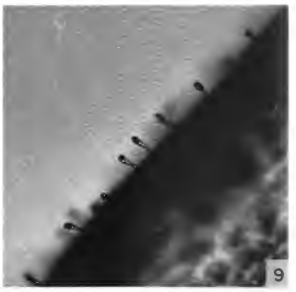


GS. 1 to 5. *Hartmannella astronyxis* Ray and Hayes. FIG. 1. Vegetative amoebae feeding on cells of *Escherichia coli* and showing characteristic filose pseudopodia, contractile vacuoles, and nuclei with centrally placed nucleoli; × 385. FIG. 2. Amoebae at the onset of the encystment stage showing the tendency of cells to form small clusters, or "aggregates"; × 25. FIG. 3. An area from figure 2 somewhat enlarged; × 160. FIG. 4. A single "aggregate"—note how the central cells are rounded and thick-walled preparatory to encystment, whereas peripheral cells are still actively amoeboid; × 385. FIG. 5. Mature cysts showing an outer circular wall and an inner thicker wall of stellate pattern; × 385. Figures 1, 3, and 4 phase microscopy.









FIGS. 6 to 9. *Sappinia pedata* Dangeard. FIG. 6. Feeding amoebae showing broad lobose pseudopodia and paired nuclei in two of the three cells; phase microscopy; × 385. FIG. 7. Amoebae seen with low magnification as they feed in a colony of *Escherichia coli* and form simple clusters or "aggregates"; × 20. FIG. 8. Large hemispherical mounds of amoebae that have moved for a limited distance through the bacterial colony, leaving cleared paths in their wake; × 20. FIG. 9. Stalked amoebae that have entered the resting stage as individual cells, forming pseudospores; × 100±.

microbial flora, the myxamoebae in great numbers develop the characteristic stalked resting stage. In continued laboratory culture, this capacity is progressively lost and after weeks or months can be restored in some measure only by recultivation in the presence of contaminating fungi, or by the addition of a small quantity of unsterile soil to provide a mixed microflora. Furthermore, and depending upon how the genus is defined, the formation of cell clusters and the development of a stalked resting stage by individual amoebae may not be confined to the genus *Sappinia* but could represent a fructifying response common to a number of relatively large, free-living soil amoebae. We have in our laboratory at the present time several cultures which possess these attributes, and some of these show the binucleate condition of Dangeard's *S. pedata* with individual nuclei containing conspicuous centrally placed nucleoli, while others have single large vesicular nuclei with multiple peripheral "nucleoli," as illustrated by Belar (1926: fig. 229) for *Amoeba terricola*. Obviously, further taxonomic and physiologic study of these stalk-forming amoebae is needed.

Here, as in *Hartmannella astronyxis*, we see a basic tendency for amoebae to form cell collectives and the evidence suggests that the prolonged existence of these aggregates, irrespective of their origin, is not a matter of chance but represents

some type of continuing intercellular stimulation and response. The capacity to form simple aggregates is thought to have developed independently in the two genera, for there is no evidence to suggest possible identity or even close relationship between *Sappinia pedata* and *Hartmannella astronyxis* as Sussman has inferred (1956) from a comparison of the illustrations in Olive's monograph (1902) and those in the paper of Ray and Hayes (1954). Olive did not work with two-membered cultures (i.e. one species of amoeba, or slime mold, growing in association with a single nutritive bacterium, yeast, or fungus); and the cyst illustrated in his figure 5, plate 5, which so closely resembles *H. astronyxis,* undoubtedly represented a contaminating amoeba rather than *Sappinia* which he accurately depicted in figures 1, 2, 3, 4, and 6, plate 5, of his monograph.

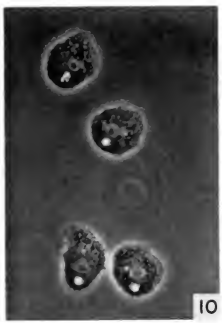
PROTOSTELIUM

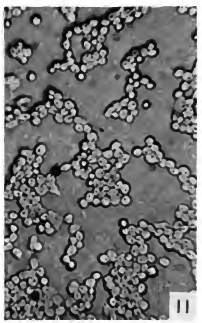
Professor Lindsay S. Olive and Carmen Stoianovitch of Columbia University have recently isolated and described a new genus and species of the Acrasieae, *Protostelium mycophaga,* which shows a simple yet definite cellular differentiation at the time of sporulation, but this is accomplished in the absence of any evident tendency for the cells to aggregate (Olive and Stoianovitch, 1960). The organism was isolated from dried florets of the marsh grass, *Phragmites communis,* and is optimally cultivated in the laboratory in association with the pink yeast *Rhodotorula mucilaginosa* on Difco cornmeal-dextrose agar enriched with 0.1 per cent yeast extract. The amoebae are uninucleate, show a contractile vacuole, and have filose pseudopodia (fig. 10), thus resembling those of the genera *Dictyostelium, Polysphondylium,* and *Acytostelium.* In dense cultures, the vegetative amoebae may appear to move and feed in small groups, as seen in figure 11, but such associations are discontinuous and apparently accidental. As the culture ages, two types of resting cells are formed. The first of these represents microcysts that form *in situ* on the agar surface, i.e. individual amoebae round up and become encased by thin protective membranes, or in older cells by secondary cellulose walls. They strongly suggest a type of resting cell found in many species of the Acrasieae (Blaskovics and Raper, 1957). The second, or sporulating stage, is unique to *P. mycophaga* in so far as known. An individual amoebae becomes hemispherical and then begins to secrete a very delicate stalk upon which it raises itself into the air (figs. 12 and 13). Upon completion of the simple stalk, which is flared at the base to provide maximal support (fig. 14), the rounded cell (or spore) rests upon its terminus by an attachment that is obviously very fragile since the spore is easily detached. If a newly formed spore comes in contact with the substratum, it may return rather quickly to an amoeboid state as seen in figures 15 and 16 without leaving any trace of a cell wall behind; in other cases, according to Olive and Stoianovitch (1960), a thin empty spore case may be left.

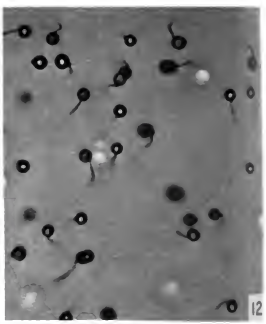
The situation in *Protostelium* is obviously quite different from that in *Sappinia.* In the latter case, the supporting stalk is in effect a narrowed but integral part of the resting cell, and upon resumption of active movement by the amoeba preparatory to feeding, the stalk, like the main body of the pseudospore, regains its fluidity and nothing is left behind. In *Protostelium* the amoeboid cell produces, apparently by secretion, a specialized support, which except for its unicellular origin is perhaps quite comparable to the secreted sorophore of *Acytostelium leptosomum* described by Raper and Quinlan (1958). Thus in Olive's and Stoianovitch's new genus *Protostelium* we witness the early beginnings of cellular differentiation, which when combined with aggregation and the multicellularity consequent to this, constitute the two essential phenomena that underlie morphogenesis in the more advanced genera and species of cellular slime molds.

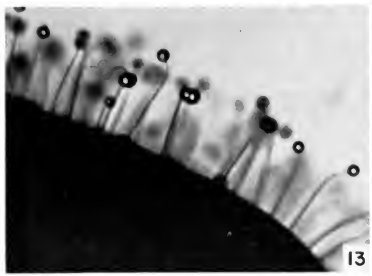
ACRASIS

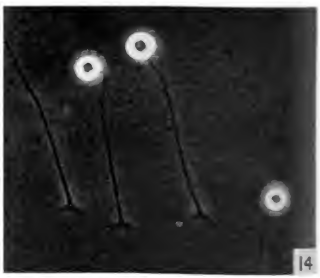
The genus *Acrasis,* described by Van Tieghem in 1880 and rediscovered only last year by Lindsay Olive and Stoianovitch, represents one of the simplest genera of the cellular slime molds wherein both aggregation and subsequent differentiation of amoebae occur to yield multicellular fructifications, albeit these are irregular and inconsistent in pattern. *Acrasis rosea* O. and S. (1960) differs from the species studied by Van Tieghem, but it possesses the same basic characteristics and should unquestionably be assigned to this genus. Like *Protostelium mycophaga,* it was isolated from dry florets of *Phragmites* and is successfully cultivated in the laboratory in the same manner. Both species thrive on *Rhodotorula mucilaginosa,* the cytoplasm of the amoebae in each case shows a faint pink or orange pigmentation, and Olive and Stoianovitch (1960) have presented evidence which suggests that the carotenoid pigment of the yeast is important to

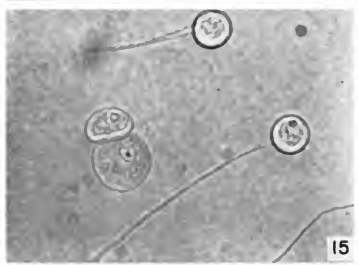












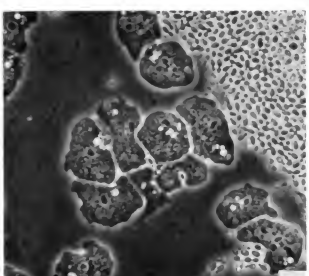
FIGS. 10 to 16. *Protostelium mycophaga* O. and S. FIG. 10. Vegetative amoebae showing short, filose pseudopodia, contractile vacuoles and nuclei with centrally placed nucleoli; × 575. FIG. 11. Amoebae at low magnification showing their irregular distribution on the agar surface at the onset of sporulation; × 190. FIG. 12. Sporulating culture viewed from above; × 260. FIG. 13. A similar culture in which a block of agar has been rotated 90° to reveal spores and supporting stems in side view; × 260. FIG. 14. Spores attached to delicate supporting stalks with expanded disklike bases, as seen with phase microscopy; × 500. FIG. 15. A newly formed spore with stalk that has been tipped over and is in contact with the underlying agar; × 600. FIG. 16. The same one hour later—note that the rounded cell has resumed an amoeboid form; × 600. Figures 10 and 14 phase microscopy.

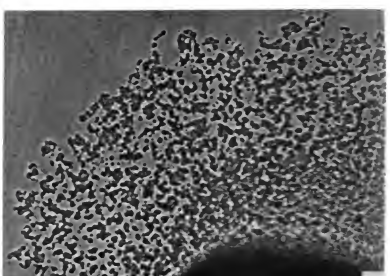
their nutrition. The amoebae of *Acrasis*, however, have lobose pseudopodia and when freely moving in water assume a conspicuous limax form. Growing on agar they assume varied shapes and dimensions, commonly ranging from 20 to 25 μ in diameter. They are uninucleate, with centrally positioned nucleoli, and like most genera of the Acrasieae possess contractile vacuoles (fig. 17). Vegetative growth occurs primarily within and at the margin of the colony of associated yeast, but after one or two days amoebae in great numbers spread onto the surrounding agar as seen in figure 18. For a few hours the cells move independently and at random before the first evidences of aggregation appear, which, as Olive and Stoianovitch have pointed out, occur most abundantly in the early morning (fig. 19). No definite streams of inflowing myxamoebae can be discerned; rather there is a generalized forward movement of cells within a limited area toward a point, or center, of greater concentration which subsequently becomes the site of the developing fructification. The community of cells, or pseudoplasmodium, collects into a rounded mass within which centrally placed myxamoebae begin to arrange themselves into a column, or stalk, and rapidly encyst. The mass of still undifferentiated cells, termed the sorogen, now begins its ascent by continually adding cells to the stalk as it rises (fig. 20). Stalks vary from uniseriate to trunklike structures many cells in thickness, and gain needed rigidity from an enveloping sheath secreted, most probably, by the cells of the ascending sorogen. Unlike the stalk cells of higher forms such as *Dictyostelium*, the constituent cells of *Acrasis* remain filled with cytoplasm and retain their capacity to revegetate when transferred to favorable environment. Upon completion of the stalk, the sorogen ceases upward movement, becomes lobed and often relobed, and the myxamoebae soon arrange themselves by rapid movement into catenulate chains which may be simple or branched (figs. 21, 22, 23). Encystment follows quickly and the whole process of cell rearrangement and spore formation from a lobe may be completed within twenty or thirty minutes. Additionally, a chain of spores may be lengthened by the interstitial movement and encystment of myxamoebae at its proximal end (Olive and Stoianovitch, 1960).

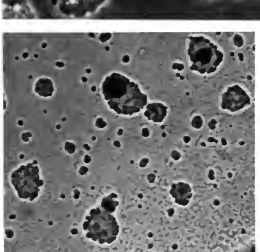
The genus *Acrasis*, as exemplified by *A. rosea*, presents the phenomena of cell aggregation and cell differentiation in about their simplest conceivable manifestations. Aggregation of myxamoebae is apparently possible only over short distances, the inflowing myxamoebae are not conspicuously oriented in the direction of the developing center, and there is at present no evidence for the outward wave-like propagation of a chemotactic stimulus such as witnessed in *Dictyostelium* and *Polysphondylium*. The level of cellular differentiation in the mature fructification is not especially marked. Cells of the stalk are not strikingly different from spores and are in fact virtually indistinguishable from the microcysts formed on the agar surface by individual unaggregated myxamoebae. The walls of spores and microcysts give a positive test for cellulose, and the former show a ringlike thickening (or hilum) at each point of contact with adjacent spores in the fructification. All differentiated cells have relatively thin walls and the protoplasts that they protect remain viable for only a few weeks under laboratory conditions. There is little to indicate that the function or ultimate position assumed by any cell during fructification represents more than a response to localized conditions and stimuli.

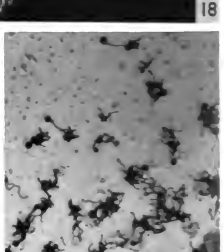
Nevertheless, *Acrasis rosea* marks a very substantial advance beyond any of the three amoeboid organisms considered previously. Here for the first time we see unequivocal multicellular collaboration leading to the formation of a unit fructification. The degree of cellular interdependence and response is of course limited for the sorocarp assumes no definite pattern; on the other hand this must be rather considerable for it does lead to the elevation of the greater portion of the population into an exposed position which affords an obvious advantage for dispersal.

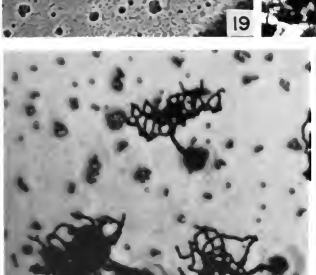
It is significant that Van Tieghem's exposition of the true nature of the pseudoplasmodium forming slime molds was based largely upon his careful analysis of *Acrasis granulata* (1880), the generic name being chosen specifically to indicate a lack of fusion among the naked cells that subsequently formed a fructification. He demonstrated that the same condition prevailed in Brefeld's *Dictyostelium* (1869) and Cienkowski's *Guttulina* (1873) and applied the name Acrasieae to these three genera characterized by a multicellular "plasmode agrégé" in contrast to other slime molds (Myxomycetes) that form a fused, large multinucleate plasmodium. *Acrasis granulata*, still to be rediscovered after eighty years, differed from *A. rosea* in certain significant details, and withal must have exhibited a greater degree of cellular differentiation. The spherical, pigmented,

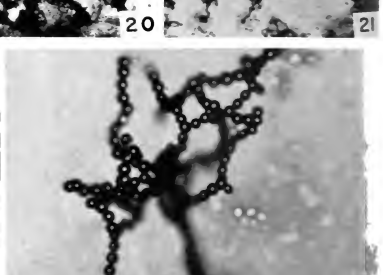












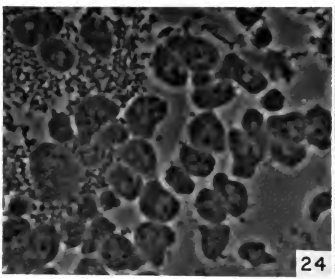
FIGS. 17 to 23. *Acrasis rosea* O. and S. FIG. 17. Myxamoebae feeding at the edge of a colony of *Rhodotorula mucilaginosa* showing characteristic lobose pseudopodia, contractile vacuoles, and nuclei with centrally placed nucleoli; phase microscopy; × 385. FIG. 18. Myxamoebae outside a yeast colony at the close of the vegetative phase; × 20 FIG. 19. A similar area after two hours wherein small, simple aggregates, or pseudoplasmodia have formed; × 2 FIG. 20. A similar area after an additional hour showing sorocarps in process of formation with bulbous sorogens from which chains of spores will develop; × 20. FIG. 21. A similar area after five hours showing completed sorocarps; × 14. FIG. 22. A group of three fructifications at greater magnification; × 72. FIG. 23. A single sorocarp to show arrangement and detail of spore chains; × 260.

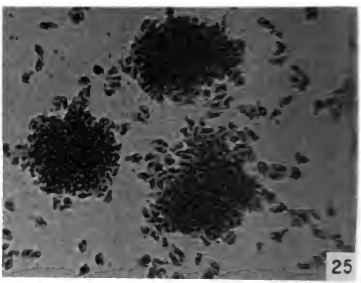
and slightly roughened spores were in fact rather strongly differentiated and probably bore little resemblance to the cellular elements of the supporting stalk. The latter consisted of a single tier of superimposed cells 1.5 to 2.0 times as long as broad, of which the basal cell was cramponlike and often buttressed by other cells applied against it. In rich cultures, compound stalks were formed that represented several adherent rows or tiers of cells, each row being terminated by a chain of spores. No mention was made of branched spore chains. Except for the fact that spores were borne in chains rather than in globose sori, we might, for the present, visualize a pattern of development rather analogous to that found in *Dictyostelium polycephalum* Raper (1957) to be considered later.

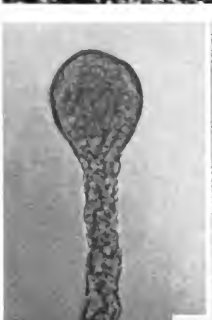
GUTTULINOPSIS

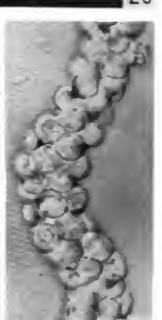
The genus *Guttulinopsis,* created by E. W. Olive (1901), is characterized by sorocarps of very different pattern from those of *Acrasis.* Yet, the degree of cellular interdependence and the level of terminal cellular differentiation are much the same. Olive described three species and considered these in some detail in his monograph of the following year (1902). Some years ago we isolated from soil and from eagle dung, respectively, two slime molds that are properly assigned to this genus. We are not certain of their specific identity and question whether either can be fitted satisfactorily into any one of Olive's taxons. Basically, their developmental cycles are similar, and in our present discussion we shall for brevity confine our attention to the culture from soil. This was isolated in 1952 by Professor Alfred E. Borg, who was at the time a guest investigator in my laboratory at Peoria, Illinois. The slime mold was obtained from a sample of leaf mold collected from a deciduous forest, and by conventional techniques (Raper, 1951) was isolated as a two-membered culture in association with *Escherichia coli.* It grows equally well upon *Aerobacter aerogenes* and can be cultivated on a variety of nutrient poor media.

The myxamoebae are small, measuring about 5.0–6.0 × 10–15 μ; typically but not consistently uninucleate (with single central nucleoli); exhibit rather explosive, lobose pseudopodia; and are devoid of a contractile vacuole (fig. 24). During rapid movement they assume an elongate limax form. Following a period of vegetative growth (commonly *ca.* two days), they collect into small aggregates much as described for *Acrasis* (fig. 25). The number of inflowing amoebae is generally greater, but the linear dimensions of the cell collective remains equally small. As in *Acrasis,* there is no indication of true stream formation, and the individual myxamoebae show no obvious orientation in the direction of the center to which they congregate. There is instead a generalized and almost imperceptible influx of cells that results in the formation of a raised central mound. As additional myxamoebae converge at this point the central mound lengthens vertically to form a narrow column, commonly 1.0–1.5 mm. high, that is generally but not consistently terminated by a slight bulbous swelling (figs. *26, 27, 28*). The cells within the column round up, forming pseudospores, but do not generally differentiate further for many hours. A thin and tenuous slime envelope that is slightly heavier toward the basal area surrounds the entire fructification. There is no suggestion of demarcation into a sorus, or spore-bearing area, and a stalk, or supporting structure—the cells throughout including the swollen terminus being indistinguishable and equally capable of immediate reversion to the amoeboid (or vegetative) state when returned to a suitable environment (fig. *29*). We do not yet know precisely how the sorocarp is formed. There is no clear evidence that cells which are still amoeboid climb up over the surface of the lengthening column, and it is rather difficult to conceive of a structure even of so small dimensions being lifted solely by the continued influx of cells beneath it. In any case, we know that the form and identity of a developing sorocarp, or even a pre-formed one, can be altered in several ways. It is commonplace for elongating sorocarps to come in contact and thereafter build only a single structure (fig. 28). Alternatively, a single column midway in formation may give rise to two or even more essentially independent branches. If erect sorocarps formed a day earlier are disorganized and their component myxamoebae disposed as a paste on the agar surface, they will immediately reinitiate aggregation with the formation of mounds and subsequently sorocarps, all within a matter of five or six hours. Any intact sorocarp of the type described will, upon being placed in water, first contract and then rapidly disintegrate as its constituent cells regain their fluidity and movement and as individual myxamoebae scatter throughout the surrounding area. Something of the same phenomenon regularly occurs in agar plate cultures, for many of the soro-









FIGS. 24 to 30. *Guttulinopsis* sp. FIG. 24. Vegetative myxamoebae feeding on *Escherichia coli* showing lobose pseudopodia, nuclei with central nucleoli, and absence of contractile vacuoles; phase microscopy; × 850. FIG. 25. Three small aggregates, or pseudoplasmodia, showing mode of formation in absence of definite streams; × 275. FIG. 26. Developing sorocarps seen from the side; × 50. FIG. 27. Mature sorocarps showing irregular pattern resulting from branching and coalescence of structures; × 50. FIG. 28. Terminal portion of a mature sorocarp as seen in figure 27, fixed in 70 per cent alcohol and iodine; × 500. FIG. 29. Terminal area of one-day sorocarp 15 minutes after being placed in water showing how the mass begins to disintegrate as the constituent cells quickly resume amoeboid movement; × 250. FIG. 30. Portion of an older sorocarp after the cells have formed firm-walled cysts or "spores"; such cells must germinate to release their protoplasts; × 1,000.

carps tend to collapse within two or three days after being formed. This is believed to result from a comparable resumption of amoeboid movement by the component cells ending in the dissolution of the previously erect fructification.

Under other conditions, not yet fully defined, a sorocarp will persist, and if such a structure is placed in water or on an agar surface its cells are seen to have developed thin but definite outer walls (fig. 30). Such a cell does not resume amoeboid movement simply by a generalized softening of its outermost surface, but the protoplast must actually dissolve a portion of the wall and emerge through the opening thus effected in order to resume free movement or vegetative growth, leaving behind a delicate, hyaline spore case.

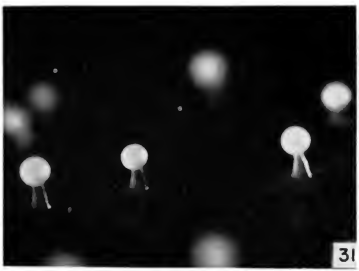
Thus in *Guttulinopsis*, as in *Acrasis*, true spores as well as pseudospores are formed. In this genus, however, the site of spore formation seems far less definite, and the phenomenon may depend, in fact, more upon external environmental conditions than upon any inherent regulatory mechanism whose genesis is within the cell community *per se*.

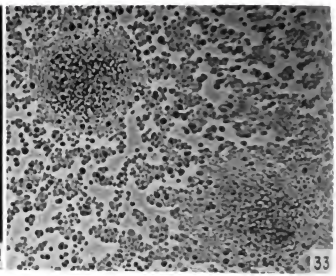
GUTTULINA

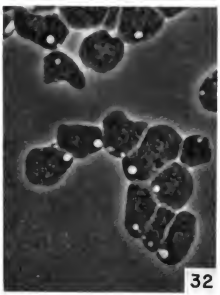
The genus *Guttulina* resembles *Guttulinopsis* in many ways but differs from it in one important regard. True spores are regularly produced, and these form a definite sorus separated from and supported by a short stalk of less strongly differentiated cells. The genus was erected by Cienkowsky in 1873; Van Tieghem enlarged it by describing two additional species (1880); while E. W. Olive recognized the validity of *Guttulina* (1901, 1902), but did not study any slime mold assignable to the genus. Nearly two years ago Dietrich Kessler, a student in my laboratory, isolated from a Panama collection of dung of howling monkey a culture which we believe to represent a new species and unquestionably belongs in this genus (fig. 31). Thus we have been able to study a type of slime mold unreported for nearly eighty years.

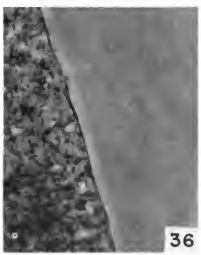
The myxamoebae are small, measuring about 6–8 × 10–18 μ when actively moving or 7-10 μ in a more quiescent state. They are generally uninucleate, and most of the nuclei show a central nucleolus; their pseudopodia are lobose and are quickly extended, resulting in rapid movement of the cell body. They contain a conspicuous and rhythmically pulsating contractile vacuole (fig. 32). Although their generation time has not yet been accurately determined, they obviously grow very rapidly since cultures from spores often initiate fructification within 24–30 hours when cultivated in association with *Escherichia coli* or *Aerobacter aerogenes* upon agar media containing low concentrations of lactose (0.2–0.5 per cent) and yeast extract or peptone (0.05–0.2 per cent). As in *Acrasis* and *Guttulinopsis*, cell aggregates are generally small and do not exhibit definite streams of inflowing myxamoebae (fig. 33). From preliminary time-lapse pictures made by Kessler, the myxamoebae often appear to form transient cell collectives and then disperse one or more times before a sufficient concentration of cells is built up and the aggregated cells proceed to form a sorocarp. The onset of this stage is first evident by the appearance of a centrally positioned mound atop a circular area of densely crowded myxamoebae. The mechanics of sorocarp formation have not been completely analyzed, and we cannot as yet trace accurately the movements and disposition of cells in the later stages of fructification. However, from the site of the aforementioned mound a short tapering vertical column of myxamoebae develops; cells from the area immediately surrounding this continue to converge and, ascending either through the column, as we believe to occur, or along its surface, collect into a terminal globose droplet where they encyst, forming small spherical spores. The sorus thus formed consists solely of spores suspended in thin slime (fig. 38), hence closely resembles the sori of *Dictyostelium* and other more advanced types.

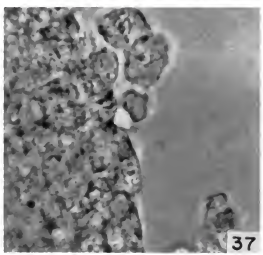
For the present, the mode of formation and true structural character of the stalk are largely unresolved. If fixed *in situ* and then examined with high magnifications, a young stalk is seen to consist of essentially undifferentiated myxamoebae packed together without definite arrangement or pattern, the whole being surrounded by a thin hyaline slime sheath (figs. 36 and 37). When similarly examined, older stalks present an inconsistent picture. Some appear definitely cellular, particularly in the terminal area; many seem to contain few undifferentiated cells, but show numerous small rounded bodies that may approximate microcysts; and others exhibit relatively few cells of any kind, but seem to consist of a type of spongelike matrix from which cells previously withdrew or within which they disintegrated. Ghosts of cells suggesting this latter fate are regularly observed in the expanded basal area of contact with the substratum. On the

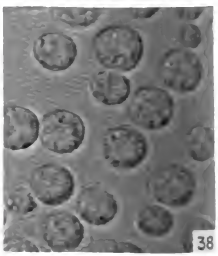












FIGS. 31 to 38. *Guttulina* sp. FIG. 31. Mature sorocarps viewed from the side showing typical globose white sporotheca borne on short tapering stalks; × 38. FIG. 32. Myxamoebae showing lobose pseudopodia, nuclei with centrally placed nucleoli, and contractile vacuoles; phase microscopy; × 675. FIG. 33. Two developing aggregate pseudoplasmodia—note that the myxamoebae converge without forming streams; × 150. FIG. 34. Stalk of a sorocarp with the spore mass, or sorus, removed; stained with 70 per cent alcohol and eosin; × 100. FIG. 35. Terminal area of stalk seen in preceding figure to which a few spores remain attached; × 320. FIG. 36. Edge of an intact stalk; × 1,500. FIG. 37. The same broken to release cells which remain essentially undifferentiated. × 1,500. FIG. 38. Mature spores; × 1,500.

other hand, the collapse of many stalks within a few hours after being formed indicate a resumption of active cell movement and withdrawal, with a consequent weakening of the vertical column as is believed to occur in *Guttulinopsis*.

Guttulina thus seems to be intermediate between *Guttulinopsis* and *Dictyostelium* in the level of cellular interdependence displayed and, more particularly, in the divergent functions and terminal differentiation achieved by the collaborating myxamoebae. Here we note the emergence of a clean-cut demarcation within the assembled population: the larger portion of the cells differentiate as spores to form a true sorus, whereas the remainder function in a supportive role, such differentiation as they display still being, obviously, at a primitive level.

There is no evidence to indicate that either *Guttulinopsis* or *Guttulina* represents imperfectly formed fructifications of *Dictyostelium* or *Polysphondylium* as Singh (1947), Cohen (1953), and Sussman (1956) have suggested, albeit species of these genera may under suboptimal culture conditions produce abnormal sorocarps that superficially resemble Olive's and Cienkowsky's genera, as was reported and illustrated for *D. discoideum* in 1939 (p. 190; fig. 3). They are unquestionably separate and distinctive genera with few characteristics that betoken close relationship to *Dictyostelium* or *Polysphondylium*: their myxamoebae are markedly different; they exhibit a more primitive pattern of aggregation; and they fail to achieve a comparable level of cellular differentiation during sorocarp formation under the best of known culture conditions.

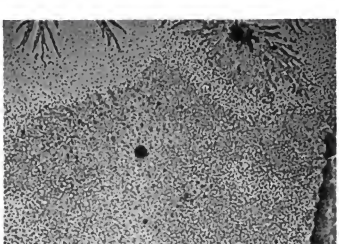
DICTYOSTELIUM

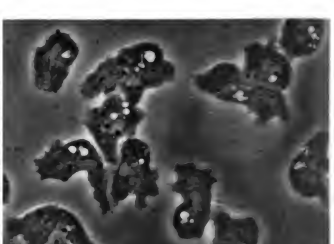
In this genus we see for the first time fully expressed the characteristics that are generally associated with the Acrasieae, or cellular slime molds, namely: (1) the development of radiate pseudoplasmodia composed of well-defined convergent streams of conspicuously oriented myxamoebae, and (2) the subsequent differentiation of this population into strongly vacuolate cells comprising sterile stalk tissue on the one hand and propagative cells (spores) on the other to yield unit fructifications of specific and heritable patterns.

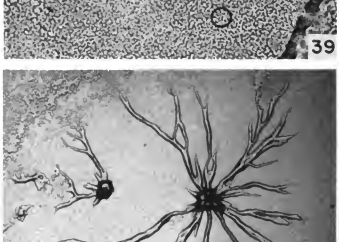
The genus was created in 1869 by Brefeld with the description of *Dictyostelium mucoroides*, which has the dual distinction of being the oldest and the most widely distributed species of the Acrasieae. Ten additional species have since been described and cultures representing most of these are presently available for purposes of comparison. Virtually all investigations prior to 1935 centered upon *D. mucoroides*, but *D. discoideum* has attracted increasing attention in the years since it was described (Raper, 1935). Whereas the developmental cycle of the two species is basically similar, the latter exhibits a migrating stage following aggregation and preceding construction of the sorocarp which commends it for many types of experiments. For this reason, and because of an obvious familiarity with it, I shall center my discussion of the genus upon this species.

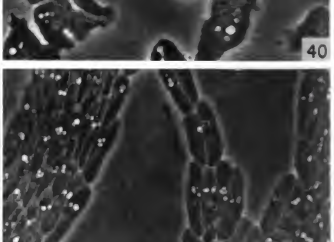
The vegetative myxamoebae of *Dictyostelium discoideum* measure about 12 to 20 μ and are generally uninucleate, the nuclei usually showing from three to five peripheral nucleoli(?). They feed and move by means of pseudopodia that are typically filose but may appear somewhat lobose in cells moving on an agar surface and especially if compressed, as under a cover glass (fig. 40); and they contain rhythmically contracting vacuoles that may be multiple in origin but merge prior to discharge (fig. 40). The myxamoebae feed by the ingestion and digestion of bacterial cells, and excellent growth can be obtained in association with Gram-negative species such as *Escherichia coli* and *Aerobacter aerogenes* upon a variety of solid culture media that may be altered to meet the needs of particular investigations (Raper, 1951). In cultures of reasonable density the life cycle is completed within 2 to $2\frac{1}{2}$ days.

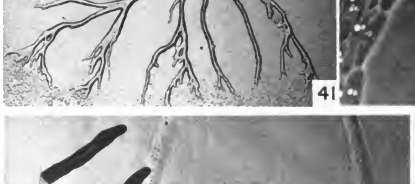
The vegetative phase is characterized by the growth and repeated division of the myxamoebae in the presence of nutritive bacteria, and during this stage each cell moves and feeds quite independently. At the close of this phase, which is usually coincident with the exhaustion of the food supply, the amoebae assume more irregular outlines and show a generally increased activity followed by a more quiescent period before becoming strongly oriented toward centers of aggregation to which they converge by individual but coordinated movements, typically forming conspicuous radiate streams as they advance (figs. 41 and 42). This phenomenon of aggregation has been studied in considerable detail, but underlying causes are not fully understood. Aggregation can be hastened by increased temperature or by exposing the culture to decreased humidity or light (Raper, 1940). Once a center has been formed the cells at this site are known to emit











FIGS. 39 to 44. *Dictyostelium discoideum* Raper. FIG. 39. Small segment of an agar plate culture showing feeding front of vegetative cells at extreme right, an early stage in pseudoplasmodium formation in central area, and portions of more advanced aggregations above; × 15. FIG. 40. Myxamoebae in pre-aggregative stage, as in area of preceding figure indicated by circle; note inconstant shape, lack of uniform orientation, irregular pseudopodia, nuclei (appearing as light gray centrally positioned areas), and contractile vacuoles (white); × 700. FIG. 41. A developing pseudoplasmodium, well advanced; the smaller organization at left resulted from severance of a major stream in the larger aggregation; × 15. FIG. 42. Strongly oriented aggregating myxamoebae in streams moving in direction indicated by arrow; × 450. FIG. 43. Migrating pseudoplasmodia moving on an agar surface and leaving trails of slime in their wake; × 18. FIG. 44. Early stage in sorocarp formation; the mass of amoebae has assumed a vertical orientation and stalk formation is just beginning; × 45. Figures 40 and 42 phase microscopy.

a chemotactic substance, termed *acrasin* by Bonner (1947), and as additional cells join the developing aggregate they too produce acrasin and attract cells peripheral to themselves. The center of the aggregate apparently retains its role as pacemaker and, as postulated by Shaffer (1958), evidence would indicate that pulses of acrasin secretion start from the center and are propagated outward through the streams by a system of relays. The probable correctness of this interpretation is strengthened by the wavelike movement of myxamoebae within the streams, a phenomenon clearly revealed by time-lapse cinematography. The chemotactic substance has not been characterized, but Dr. Barbara Wright (1958) demonstrated that certain steroids, e.g. estrone sulfate, could induce aggregation in *D. discoideum,* and Wright and her collaborators (Heftman, *et al.,* 1959) have isolated from the same species a sterol (Δ^{22}-stigmasten-3β-ol) that possesses acrasin-like activity. Acrasin is known to have a very short half-life and to be destroyed by an enzyme (Shaffer, 1956) which the acrasin secreting cells themselves produce. By virtue of acrasin production, acrasin destruction, and its wavelike propagation in an orderly and sequential manner, together with coincident changes in the cell surfaces that cause them to adhere to each other, an overall guidance is maintained to attract and regulate the inflowing myxamoebae (Shaffer, 1958).

Initiation of the aggregative process has interested most students of the cellular slime molds. We know that the inductive stimulus first appears following exhaustion of the food supply, that it originates within the cell population and is species specific within certain limits (Raper and Thom, 1941; Shaffer, 1953 and 1957), and that it either reflects or is the product of physiological changes in the myxamoebae themselves. Professor Maurice Sussman and associates have published a series of papers, dating from 1952, wherein they report aggregation to stem from the presence of special Initiator cells (I-cells) which occur in their populations, and under their experimental conditions, in a ratio of about 1:2,000. Such cells were first postulated upon the basis of the aggregative capacity of populations of centrifuged cells dispersed at controlled densities on washed agar. More recently they have reported them to possess distinctive morphological characteristics as well, the I-cells being substantially larger and more active than the remainder of the population, i.e. responder cells.

Under their special conditions, which need not be detailed here (see original papers by Sussman *et al.,* 1952, *et seq.*), they report a high positive correlation to exist between the presence of an I-cell and the capacity of a small population to form an aggregate. In other laboratories to date, investigators have not succeeded in demonstrating large and especially active cells in their populations at the relatively definite ratios reported, nor have they found evidence of a positive correlation between the presence of such cells and the capacities of small isolated populations to form aggregates. The presence of specially endowed cells may represent one of a number of factors responsible for, or influencing, the phenomenon of cell aggregation in *D. discoideum;* undoubtedly, the physiological state of the preaggregative test population is important, as are also factors arising from the physical environment. The need for further work in this area is obvious.

As the myxamoebae converge at the center of the aggregate, they collect into a peglike column which is at first vertical, but in *Dictyostelium discoideum* soon assumes a horizontal orientation and moves as a cartridge-shaped unit along the agar surface (fig. 43), responding to a variety of stimuli, including light and increased temperature. This migrating pseudoplasmodium is a very interesting structure. It consists of many thousands of separate but closely integrated cells, each with its own motility, coordinated in such a manner that the moving mass keeps its form and in many ways functions as a single multicellular organism. Stimuli that influence direction of movement are received at the apex of the moving body, and the movement of the whole is directed from this site. It is not known to what extent these receptive and directive capacities reside within special groups of cells, or whether they stem instead from specialized areas within which the identity of the cells is constantly changing. In any case, migrating pseudoplasmodia show a forward area of presumptive stalk cells and a larger, more posterior area of presumptive spore forming cells; and interesting experiments can be performed by grafting different parts of the pseudoplasmodia in various ways and following the developmental pattern in the sorocarps that emerge from these (Raper, 1940; Bonner, 1952).

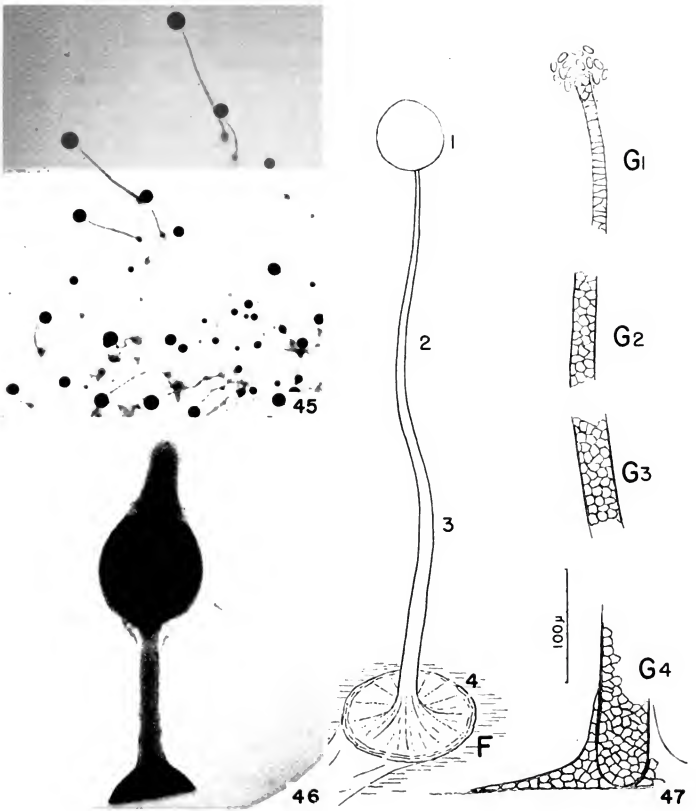
The details of sorocarp formation in *Dictyostelium discoideum* have been worked out in considerable detail by Raper and Fennell (1952) and the reader is referred to their paper for more complete information concerning this process. A

very brief account is sufficient for our present purposes. In time, the migrating pseudoplasmodium ceases forward movement and assumes a rounded form surmounted by an apical papilla (fig. 44). In the center of the mass and just beneath this papilla a thin, hyaline cellulose tube begins to be laid down. The diameter of the tube is beautifully proportioned to the size of the mass and the cells inside and outside this tube are still undifferentiated at this stage. As culmination proceeds cytological evidence indicates that it is the external cells which are largely responsible for its formation (Bonner, *et al.*, 1955), but at this early stage the whole community of cells must in some way be responsible for its location and dimensions if not its actual formation. The tube is first extended downward to the substratum where it becomes closed at the bottom. It is then constructed upward and with its continued extension, the mass of undifferentiated cells, or sorogen, is drawn upward with it. As sorocarp formation proceeds a clear separation into zones of stalk-forming and spore-forming myxamoebae becomes evident (fig. 46) and the process continues until all of the cells have differentiated to form either the vacuolated and compacted elements of the supportive stalk or the encapsulated spores that comprise the sorus. The completed sorocarp of *D. discoideum* when formed under optimal conditions is a beautifully proportioned structure consisting of an upright tapering stalk fitted into a flattened cuplike basal disk below and bearing a globose to slightly citriform spore mass at its terminus (fig. 47). The size of the structure is clearly determined by the number of myxamoebae contributing to its formation, and may contain only a few hundred cells (or less) in diminutive sorocarps or upward of two hundred thousand in the largest fructifications. In any case, the dimensions of its parts are proportional one to the other (fig. 45), and we must conclude from this that the building process is controlled by the continuing interaction of the whole body of myxamoebae functioning as a unit organization. Evidence for such community control is provided additionally by the fact that individual myxamoebae when differentiating as stalk cells assume whatever shape is required locally to achieve this overall proportionality (Harper, 1926; Raper, 1935, 1941*b*).

Other species of *Dictyostelium*, for the most part, develop in much the same manner as *D. discoideum*. In such species *as D. mucoroides*, and *D. purpureum* Olive (1901) formation of the sorocarp originates at the site of aggregation and the fruiting structure is itself built towards the light. Under conditions of one-side illumination, the stalk of such a sorocarp may reach a length of several centimeters, the greater portion of this being formed adjacent to the substratum before the sorogen assumes a vertical orientation and constructs the terminal section of the fructification more or less vertically into the air. In very small species, such as *D. lacteum* Van Tieghem (1880) and *D. minutum* Raper (1941*a*), single aggregates regularly give rise to multiple sorocarps, the dimensions of these fructifications being such that clear responses to light are not generally evident.

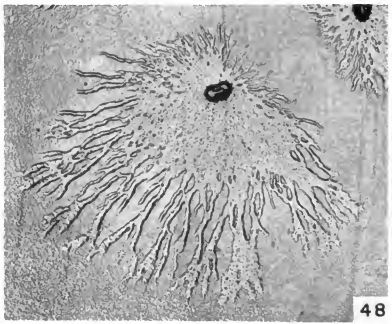
In the single species *Dictyostelium polycephalum* Raper (1957) the myxamoebae aggregate in much the same way as in the species already considered but as aggregation proceeds the cells collect into a number of long, thin, and tenuous pseudoplasmodia which are capable of migrating for variable distances before sorocarp formation takes place (figs. 48 and 49). The movement of such pseudoplasmodia is apparently undirected by any known stimuli. Upon cessation of migration, such a pseudoplasmodium rounds up and from the surface of this body a number of papilla emerge. Stalk formation begins concurrently within each of these (fig. 50), and for a considerable period of time thereafter the cluster of resulting sorogens synchronously build individual but adjacent sorophores (fig. 51). When these have attained about three-fourths of their ultimate length, they diverge sharply and the myxamoebae surrounding each developing sorophore differentiate as spores to form a small globose sorus (fig. 52). Thus, in *D. polycephalum* we see not only evidence of continuing cellular interaction governing the behavior and terminal differentiation of the cells comprising a single sorogen, as in *D. discoideum*, but in addition we note that the building processes in contiguous daughter sorogens proceed at a uniform rate, indicating a considerable measure of inter-populational communication and control as well.

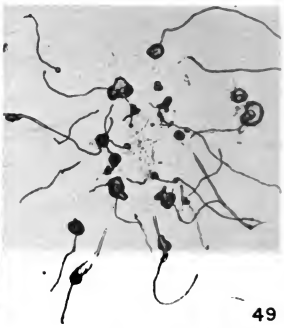
One of the most striking features of the developmental process in *Dictyostelium* is the subdivision of the population of initially equipotential myxamoebae into a class of cells that ultimately differentiates into spores, and so retains its capacity to regenerate, and into another class that loses this capacity when it differentiates into the strongly vacuolate cells that comprise the stalk. In some recent investigations, Dr. Whittingham



IGS. 45 to 47. *Dictyostelium discoideum* (continued). FIG. 45. Sorocarps of varying dimensions viewed obliquely—note that the proportions of sorus to stalk length and diameter are relatively constant irrespective of sorocarp size; × 8±. FIG. 46. A developing sorocarp fixed and stained to reveal the sharp demarcation between pre-stalk cells in apical area and the spore and pre-spore cells that comprise the main body of the sorogen; × 120. FIG. 47, F. Diagrammatic representation of a mature sorocarp showing the expanded basal disk, the tapering stalk, or sorophore, and the terminal spore mass, or sorus. G1 to G4. Detail of cellular structure at levels indicated by numerals in F; in the absence of any enveloping membrane the spores comprising the sorus have floated free, leaving the stalk terminus bare. (Figure 46 after Raper and Fennell, 1952: figure 7. Figure 47 after Raper, 1951: figure 1.)

and I (1960) had occasion to examine the site at which this loss of viability actually occurs in *D. purpureum*. It was found to take place near the terminus of the developing sorophore and could be localized within the expanded funnel-like area where rapid vacuolation of the stalk cells is taking place. By careful dissection of this area and the removal of single cells from within the





FIGS. 48 to 52. *Dictyostelium polycephalum* Raper. FIG. 48. Developing pseudoplasmodium—note transverse banding in peripheral areas of streams that results from waves of converging myxamoebae; × 10. FIG. 49. Migrating pseudoplasmodia moving away from the aggregation site; × 15. FIG. 50. A cluster of sorogens from a single migrating body; stalks are beginning to be formed; × 60. FIG. 51. A cluster of sorocarps in process of formation; × 52. FIG. 52. A mature coremiform fructification consisting of seven sorocarps with separate but adherent stalks except in the terminal area; × 135. (Figures in part selected from Raper, 1957: pls. 2 and 4.)

funnel, it was found that these lost their viability once they had become strongly vacuolated, but prior to the time when they become cemented together to form integral structural elements of the rigid stalk.

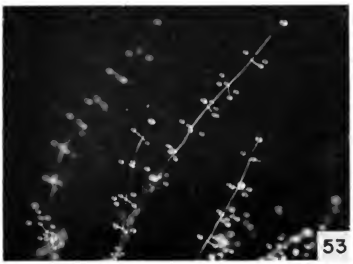
POLYSPHONDYLIUM

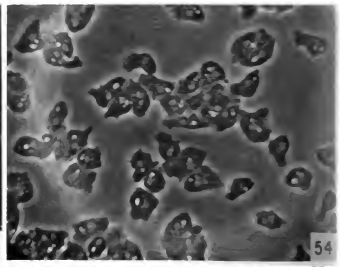
The genus *Polysphondylium* was established by Brefeld with his description of *P. violaceum* in 1884. Whereas this species is the most commonly encountered representative of the genus in nature, there are, in addition to it, certain white-spored forms which we regard, for the most part, as *Polysphondylium pallidum*, a species described by E. W. Olive in 1901 (fig. 53). The latter species tends to develop sorocarps of more regular and consistent pattern than does *P. violaceum*, and for this reason we shall use it as the principal object for our discussion of this genus.

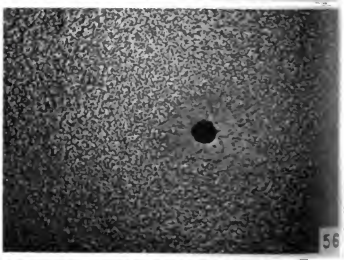
The myxamoebae are quite similar to those found in species of *Dictyostelium* (fig. 54), and growth occurs under much the same cultural conditions except that *P. pallidum* cannot be cultivated optimally on substrates containing high concentrations of nutrients. With the exhaustion of the bacterial food supply, the myxamoebae form wheel-like aggregates as in *Dictyostelium* (figs. 56 to 60). These, however, show certain distinctive characteristics. The inflowing streams, are at first strongly anastomosed, but as they develop tend to become discrete and terminate abruptly at their distal ends. According to Shaffer, those of *P. violaceum* surround themselves with a thin slime covering at this stage.

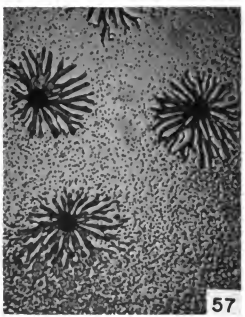
In so far as I know, no one has investigated the onset of aggregation in *P. pallidum*, but Shaffer has given attention to the phenomenon in *P. violaceum* (personal communication). Working with very small populations grown *in situ*, he has found that from among a group of cells quite indistinguishable from one another, a previously unidentifiable myxamoeba will round up and to this the neighboring cells converge immediately and in dramatic fashion. If such a small aggregate is then disrupted, as by jarring the preparation, so that all the cells are quite separate from each other, they will within a few seconds again converge at the site of the rounded cell. Having attained the capacity to attract neighboring myxamoebae, this so-called *founder cell* starts the aggregative process. In origin and properties this cell is quite unlike the I-cell reported by Sussman for *Dictyostelium discoideum*, and analogy as to function is questionable. The founder cell exercises its inductive capacity immediately and to its precise position in the population; the I-cell may perform its inductive role 10–15 hours before actual cell aggregation occurs and the center of the aggregate when formed may but need not be at the site of this special cell. The nature of the chemotactic substance in *P. violaceum* is thought to resemble rather closely that of *Dictyostelium discoideum*. However, the acrasins produced by the two species are sufficiently different so that in mixed populations the myxamoebae respond to specific stimuli emitted by cells of their own kind and overlapping streams are observed as the two types of myxamoebae aggregate to separate centers (Raper and Thom, 1941).

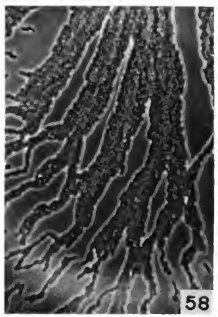
As aggregation nears completion, a single vertical column of cells develops at its central point *if* the pseudoplasmodium is small. If it is larger, a variable number of such columns will appear and from each of these a separate sorocarp will develop, although in crowded cultures the stalks of two adjacent structures may remain more or less adherent (figs. 61 and 62). In *Polysphondylium pallidum* we now encounter still a different order of cellular coordination and subsequent differentiation. As the sorogen begins to rise on the developing sorophore, much as it would for example in *Dictyostelium mucoroides*, small masses of posteriorly positioned cells cease their upward advance and remain fixed on the stalk, while the greater portion of the sorogen advances with the continued elongation of the main axis. Soon thereafter several papillae evenly spaced around the outer edge of the severed cell mass make their appearance, and within each of these stalk formation is initiated and proceeds in a direction roughly perpendicular to the main axis (figs. 63 to 65). The stationary mass now becomes transected and each developing side branch carries a diminutive sorogen within which stalk formation and subsequently spore differentiation takes place just as it does in a tiny sorocarp of *Dictyostelium*, or for that matter, in a diminutive and unbranched fructification of *Polysphondylium*. The whorl of side branches thus formed are of course anchored to the main axis, and the manner of their attachment to this corresponds to that of the main stem to the substratum (fig. 67). This performance is repeated at about hourly intervals, or a little less, with the result that the final fructification may consist of as many as eight or ten regularly spaced whorls of branches, the completed structure producing the

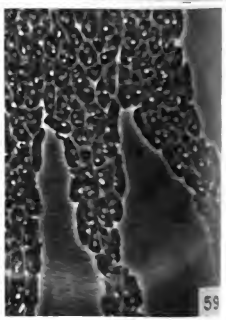




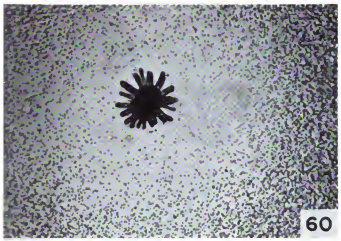


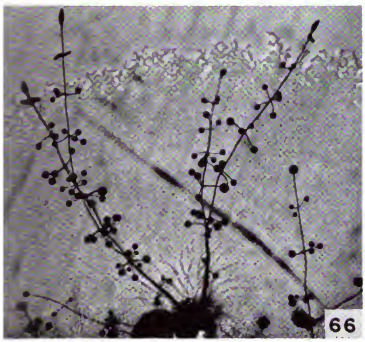


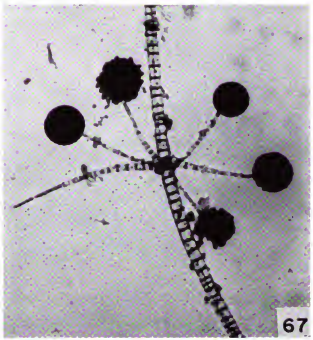




FIGS. 53 to 59. *Polysphondylium pallidum* Olive. FIG. 53. Typical sorocarps photographed with reflected light to reveal their natural habit and appearance; × 8. FIG. 54. Vegetative myxamoebae—nuclei appear as small dark areas near cell centers while contractile vacuoles appear white; × 430. FIG. 55. A dense and uniform growth of myxamoebae prior to the onset of aggregation; × 12. FIG. 56. Comparatively early stage in pseudoplasmodium formation; × 12. FIG. 57. More advanced stages; × 12. FIG. 58. Peripheral area of a developing pseudoplasmodium; × 100. FIG. 59. Detail of converging streams showing uniform orientation of the myxamoebae as they move in the direction indicated by arrow; × 375. Figures 54, 58, and 59 phase microscopy.







s. 60 to 67. *Polysphondylium pallidum* (continued). FIG. 60. Late stage in pseudoplasmodium formation and prior to emergence of sorogen(s); × 12. FIG. 61. Three young sorocarps arising from a single aggregation site; × 18. FIG. 62. A cluster of young sorocarps viewed from the side; × 18. FIGS. 63, 64, 65. Progressive stages in sorocarp formation photographed at hourly intervals—note development of whorls of side branches in structure at right; × 18. FIG. 66. Sorocarps in terminal stages of development, photographed with transmitted light; × 8. FIG. 67. Enlarged view of a single whorl showing the manner in which the side branches are attached to the main stem; × 175.

aspect of a miniature pine tree, as Professor Harper so aptly described that of the related species, *P. violaceum*, in 1929 (figs. 53 and 66). The species *Polysphondylium violaceum*, which has been studied far more than that which I have discussed, differs from *P. pallidum* primarily in the pale violet color of its sori (and to a lesser degree its stalks), its larger dimensions, and its more irregular and rangy habit. Basically the patterns of growth and of development in the two species are much the same.

How does one account for the formation of such a beautifully symmetrical, branched structure by a population of separate but obviously intercommunicating cells. Could it be that the building process proceeds under some system approximating hormonal (or acrasin) control centered in the apical region but of diminishing strength so that periodically a small fraction of the total mass is cast off in order to restore a proper balance within the main body of the rising sorogen? Whereas the strength of residual guidance in the posteriormost faction might be low with reference to the larger terminal cell mass, it might still be relatively high in terms of the small immobile mass and quite sufficient to govern a continuation of the differentiative processes in the small side branches. In any case, the sorocarp of *Polysphondylium* suggests yet another advance in the level of cell interactions required to regulate cellular behavior and differentiation to achieve this relatively complex community objective.

ACTYOSTELIUM

This very delicate but most interesting genus was described two years ago. The dimensions and pattern of the myxamoebae are much the same as those found in *Dictyostelium* and in *Polysphondylium* (fig. 71). The only known species, *Actyostelium leptosomum* (Raper and Quinlan, 1958), can be cultivated satisfactorily in association with *Escherichia coli* or *Aerobacter aerogenes* on agar media of low nutrient content containing a sugar such as a lactose or glucose and a nitrogen source such as peptone or yeast extract. Again the onset of the fruiting phase follows soon after the depletion of the available food supply. The myxamoebae collect into typical wheel-like aggregates from which multiple sorocarps regularly develop (figs. 68, 70, 72), and some of the latter may be well advanced while streams of myxamoebae still converge and subsequently give rise to additional sorocarps.

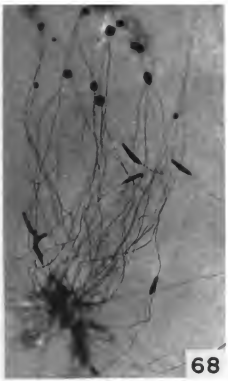
It is the sorocarps in particular which distinguish this genus and species from all others. Unlike the cellular sorophores seen in *Dictyostelium* and *Polysphondylium*, the stalks of *Actyostelium* contain *no* cells but represent extremely delicate cellulose structures formed by ascending sorogens during fructification (fig. 73). Such sorophores, terminated by small globose sori, may be as much as a millimeter and a half in length but do not exceed two microns in diameter (figs. 68, 69, and 74).

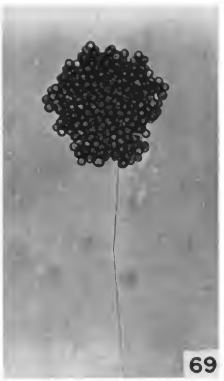
As first observed by Shaffer, the myxamoebae of this genus cannot normally aggregate unless exposed to air, or unless they are beneath or near a site where aggregation occurs. Employing test systems where the myxamoebae were grown between thin agar layers, it has been possible to demonstrate that the aggregative process can be induced by the application of certain alkaloids, e.g. yohimbine tartrate, in very low concentration (Hostak, 1960). It is not presumed that the alkaloids are responsible for this phenomenon *per se;* rather, it is expected that they stimulate or contribute to the formation of a specific, self-generated acrasin-like chemotactic agent that causes the myxamoebae to aggregate.

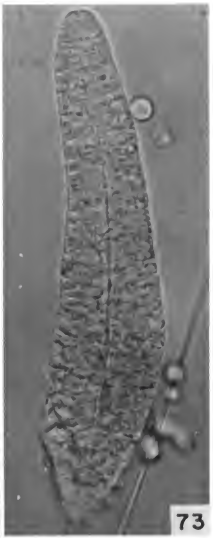
We see in *Actyostelium* a type of slime mold that achieves a multicellular fruiting structure in which none of the cell population is sacrificed in order to raise the spore mass into the air. The stalk is formed near the apex of the sorogen and the orientation of the surrounding myxamoebae suggests an origin comparable to that of the hyaline cellulose tube in *Dictyostelium*, but with this important difference: No cells are trapped within it to become strongly vacuolate and sterile but all form spores and so retain their propagative capacity. Mrs. Quinlan and I regarded this as representing a cellular slime mold of the most advanced type. Bonner (1959), on the other hand, considered it to be relatively primitive, and Olive and Stoianovitch (1960) see in this a projection of their genus *Protostelium* at the multicellular level.

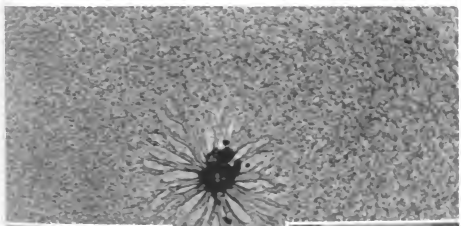
COENONIA

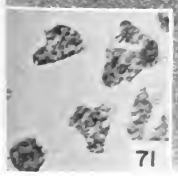
There remains for consideration one additional described genus of the Acrasieae which we believe to be the most remarkable of all. Van Tieghem in 1884 reported in considerable detail, but did not illustrate, a genus and species, *Coenonia denticulata*, which possessed at least three developmental features of singular significance. The base of the sorophore consisted of a multicellular crampon, or hold-fast; the spores (soru-

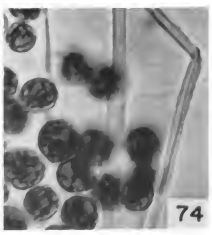












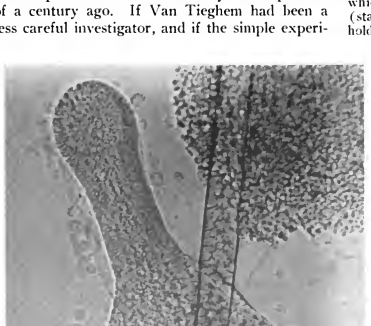
FIGS. 68 to 74. *Acytostelium leptosomum* R. and Q. FIG. 68. A cluster of sorocarps arising from a single pseudoplasmodium such as that seen in figure 70; × 22. FIG. 69. A single sorus (stained with erythrosin in aqueous phenol and flattened) surmounting a very delicate, acellular *sorophore*; × 420. FIG. 70. A developing pseudoplasmodium surrounded by a dense and uniform population of myxamoebae; × 35. FIG. 71. Unaggregated myxamoebae (stained) such as occur in the marginal areas of figure 70; × 850. FIG. 72. Detail of a converging stream of myxamoebae (stained) such as comprise the pseudoplasmodium shown in figure 70; × 715. FIG. 73. A developing sorocarp—note the transverse orientation of the constituent myxamoebae and the extension of the delicate sorophore into the apical region; × 750. FIG. 74. Mature spores, stained, and two *sorophores*; × 1,400. (Figures selected from Raper and Quinlan, 1958, pls. 1, 2, and 4.)

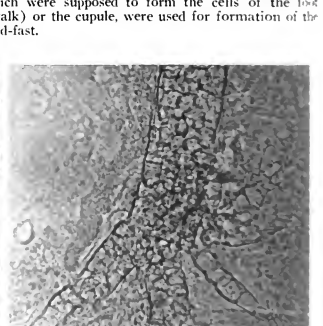
were borne within a terminal cupule-like dilation of the stalk, or of the main axis and each side branch in larger fructifications where the latter occurred; the peripheral cells of the stalk each showed on its external face a small protuberance in the form of an upturned tooth, the marginal cells of the sorus-bearing cupule were each prolonged into an even longer tooth, while extensions of the membranes of the topmost cells projected between the spores as fine, rather long and regular teeth! Obviously a fructification of this type could hardly be produced by morphogenic processes as we know these from genera and species of the Acrasieae now in our possession. The formation of a cupule with spores borne within it is quite in contrast to the simple terminus of a stalk of *Dictyostelium*, or the branches of *Polysphondylium*, with their spores suspended in adherent slime droplets. The level of differentiation achieved by the individual cells of the supportive structure must surpass anything known in these other genera. The ramified hold-fast is quite different from but may be analogous with the basal disk formed in *D. discoideum*.

Unfortunately, this remarkable genus has not been reported since its discovery three-quarters of a century ago. If Van Tieghem had been a less careful investigator, and if the simple experiments he reported had been less skillfully done and less perceptively interpreted, we might question his account. Such is not the case. Obviously, he had a very clear picture of the nature of the Acrasieae, and he could not possibly have written as he did of *Cocnonia* without having carefully observed a cellular slime mold quite different from anything studied since his time. The closing paragraphs of his paper, translated and presented herewith, bear ample witness to this fact:

In this organism, what seems to me of interest and of value in general biology, is the profound differentiation of its fructification: the fruit is formed by the simple aggregation of identical, previously free, cells. the differentiation of a myxamoeba only depends on the relative position it occupies in the whole mass. The first hypothesis is obvious; the second can be easily demonstrated.

At the beginning of the aggregation, after the formation of the hold-fast, one can remove with a needle most of the superposed plasmodium (sorogen) without removing the hold-fast and one can replace this plasmodium in the nutrient drop. The heap will be formed again, by first reforming the hold-fast then a complete fruit smaller than in the normal case. It is obvious that a certain number of myxamoebae, which were supposed to form the cells of the foot (stalk) or the cupule, were used for formation of the hold-fast.





FIGS. 75 and 76. *Dictyostelium* sp., undescribed. FIG. 75. Terminal portion of a developing sorocarp showing the apical region wherein the stalk is forming, and a portion of a mature stalk consisting of strongly vacuolated cells each derived from a single myxamoeba; × 325. FIG. 76. The base of the stalk showing a crampon, or hold-fast of a type believed to approximate that described by van Tieghem for the genus *Coenonia*; × 325.

Later, after the column has more or less reached its definitive dimensions, one can remove anew the terminal, spherical, still uncolored globule, and replace it in the nutrient medium. A new hold-fast, then a complete but much smaller fruit will be formed. A certain number of myxamoebae which were supposed to become spores were obviously used for producing the cells of the hold-fast and the foot.

This independence and indifference of the constitutive elements which do not prevent a high differentiation confer to the family Acrasieae a great biological interest which will become greater and greater with further observations of these peculiar organisms.

Further evidence of the correctness of Van Tieghem's report, if such were needed, is strongly suggested by the recent discovery of a new and still undescribed species of *Dictyostelium*, isolated from a sample of Panama soil, which shows a ramified hold-fast of the type described by him. Thus we have, in a sense, rediscovered the base of a *Coenonia* fructification, but the more interesting and significant examples of cellular differentiation exhibited by its aerial parts still elude us, for the isolate now in our possession shows only the adherent sorus and the untoothed stalk cells of a *Dictyostelium* (figs. 75 and 76).

EPILOGUE

Thus do the cellular slime molds play out their simple roles; and in so doing they exhibit a number of variations on two central themes. *First*, the aggregation of previously independent myxamoebae to form cell communities is characteristic of all species, and these communities, or pseudoplasmodia, attain differing levels of cellular interdependence ranging from the loose and transient associations often seen in the more primitive types such as *Guttulinopsis* to the highly organized migrating pseudoplasmodia of *Dictyostelium discoidcum* that move as unit structures and respond to external stimuli in much the same manner as true multicellular organisms. *Second*, the subsequent differentiation of such associated cells ranges from almost negligible in the simplest forms to a clear-cut separation into propagative spores on the one hand and strongly modified, sterile supportive cells on the other in forms that are more advanced. Not least remarkable is the fact that this differentiation is at all times under the direct and continuing control of the community of interdependent and still undifferentiated myxamoebae, a community (or communities in *Polysphondylium* and *D. polycephalum*) that decreases progressively in mass and numbers as fructification proceeds until in fact all cells become integral parts of beautifully proportioned structures of delicate and exquisite design. These organisms are obviously simple, and the levels of cellular interaction that lead to divergent cell differentiation even in the more advanced forms must also be relatively uncomplicated as compared with higher plants and animals. Yet, it is our belief that the cellular slime molds may teach us much about many vital phenomena if we can but pose the proper questions, design the correct experiments, and comprehend what they reveal concerning cellular interactions and responses throughout their developmental history.

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THE WINGS OF INSECTS AND BIRDS AS MECHANICAL OSCILLATORS

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President, E. I. du Pont de Nemours and Company

IN the course of a study of the flight of hummingbirds, I have taken several hundred high-speed moving picture sequences of the female ruby throat (*Archilochus colubris*) under a great variety of flight conditions. The sequences include hovering flight, flight at linear velocities up to thirty miles per hour—the top speed of which these birds are capable—take-offs from a perch, and escape routines when the birds became frightened. For all of these conditions the wing beat frequency remained constant at 53 ± 3 wing beats per second.

The constancy of wing beat rate pointed strongly to the possibility that these frequencies for hummingbird wings, and perhaps for insects and birds generally, could be explained as characteristic frequencies of driven damped oscillators.

The motion of such oscillators, assumed to be harmonic, is described by the following differential equation ($\dot{\varphi} = d\varphi/dt$, etc.).

$$I\ddot{\varphi} + B\dot{\varphi} + f r = F_0 r \cos \omega t, \tag{1}$$

where

- I is the moment of inertia of the oscillator,
- B the damping parameter, which accounts for the external and internal damping resistance assumed to be proportional to the angular velocity of the oscillator,
- f the harmonic restoring force,
- F_0 the amplitude of the driving force,
- r the effective radius of attack of f and F_0,
- ω the frequency of the driving force ($\omega = 2\pi\nu$, where ν is the number of oscillations per unit time),
- φ the angular displacement, and
- t time.

In applying this model to birds or insects, I is to be identified with the sum of the external and internal moments of inertia. The external moment of inertia is that of the wing I_w, the internal, I_{int}, that of the muscle and whatever portion of bone, tissue, and external skeleton oscillates with it. B is the damping parameter due to internal "friction" and to external air resistance. $F_0 r$ is the amplitude of the moment exerted by the muscular driving force derived from muscle chemistry.

The restoring moment fr must be related to the elastic properties of muscle tissue. Let us assume that the muscle elasticity can be described by the force-extension relation of elastic polymers such as rubber, in the Gaussian-network approximation.

$$f = G_0 .1_0(\beta - \beta^{-2}), \tag{2}$$

where G_0 is the elastic modulus of the muscle, $.1_0$ the cross section of the unstrained muscle, and β the extension ratio b/b_0, where b and b_0 are the length of the strained and unstrained muscle relative to the equilibrium position $\varphi = 0$, respectively.[2] For small extension ratios, the expression $(\beta - \beta^{-2})$ reduces to $\sim 3\Delta b/b_0$, so that with $G = 3G_0$,

$$f = G.1_0 \frac{\Delta b}{b_0}. \tag{2a}$$

We assume further that $.1_0$ is proportional to b_0^2, and Δb to $r\varphi$. Then,

$$f = K b_0 r \varphi, \tag{3}$$

where K is a proportionality constant.

The characteristic frequency of the system described by eq. (1), when free and undamped, i.e., when $B = 0$ and $F_0 = 0$, is given by eq. (4).

$$\omega_0^2 = \frac{K b_0 r^2}{I}. \tag{4}$$

The work, W, done by the driving force F_0

[1] I must express my deep appreciation to Dr. Werner Brandt who undertook cheerfully the difficult and I fear unrewarding task of instructing me in the mathematics of oscillator theory, and who made many other helpful contributions in the preparation of this paper.

[2] Recently, Machin and Pringle (1959) reported experimental force-extension curves of insect muscles which in fact can be interpreted over their entire range in terms of the theory of rubber elasticity for non-Gaussian networks, applicable for strains up to the maximum extension of a molecular network. For moderate strains, eq. (2) follows directly from this theory.

against the damping resistance, is

$$W = \int B\dot{\varphi}d\varphi = \Phi_0^2 B\omega^2 \frac{t}{2} \qquad (5)$$

where t is long compared to the time for a single wing beat. Φ_0 is given by eq. (6)

$$\Phi_0 = \frac{F_0 r}{[I^2(\omega_0^2 - \omega^2)^2 + B^2\omega^2]^{1/2}}. \qquad (6)$$

Combining eqs. (5) and (6)

$$W = \frac{F_0^2 r^2 B\omega^2 t}{2[I^2(\omega_0^2 - \omega^2)^2 + B^2\omega^2]}. \qquad (7)$$

The mechanical work is thus dependent only on amplitude and frequency of the wing beat, and on the damping parameter B corresponding to a given flight regime. However, the muscular driving force F_0 will be at a minimum when $\omega = \omega_0$. Since provision of the force F_0 is certainly associated with energy expenditure in the muscle, it follows that muscular energy will be at a minimum when the muscular driving force is in resonance with the characteristic frequency of the undamped system. Because of eq. (4), the wing beat rates of insects and birds should be essentially independent of both external and internal loading.

As direct evidence that wing beat frequencies are indeed unaffected by the external load, we cite (1) the results with the female ruby throat described in the opening paragraph, (2) experiments with insects flying in air at various subatmospheric pressures which show little or no change in wing frequency as the air pressure (external wing loading) is reduced.[3]

We take these observations to confirm that in general, for maximum economy in energy expended, the wings of flying insects and birds beat at the characteristic frequency of the undamped system, regardless of loading.

Sotavalta (1952 and 1954) gives a mass of valuable data which permit a conclusive test of the validity of eq. (4) for the unarticulated wings of insects. For a large number of insect species he clipped progressively larger pieces from the wings of a given insect and measured both wing frequency and moment of inertia of the residual wing segment. He carried the mutilation process to the point that wing segments at the end of a series of experiments were vestigial.

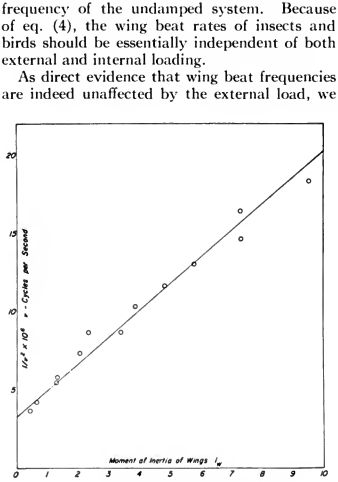

FIG. 1. Data from Sotavalta (1952) for two specimens of *Apis mellifica*.

[3] Sotavalta (1952) finds an increase in wing frequency with decreasing air pressure for some insects; for others there is no effect. Where the effect exists it is small, never more than a 15 per cent increase in wing frequency for a decrease in air pressure from 1 to 0.1 atmosphere. This behavior also is in accord with oscillator theory, if we assume W and Φ constant and $F \leq F_0$. From eqs. (5) and (7) we derive the expression

$$\frac{1}{c(c-1)} \leq \frac{I^2\omega_0^2}{B\omega^2},$$

where $c \equiv \omega^2/\omega_0^2$ and the subscript zero refers to the resonance condition. One infers from this equation that the larger the value of the right-hand term the more closely the insect or bird is bound to its resonance frequency. Now if $I \sim l^5$ and $B \sim l^4$, the larger $\omega_0 l = 2\pi r_0 d$ the smaller the permissible departure from resonance without exceeding F_0. The following table is based on Sotavalta's data.

Insect	$r_0 d$	Effect of reducing atmospheric pressure
Apis mellifica	2,390	None
Bombus terrestris	2,700	None
Vespa vulgaris	1,830	None
Vespa germanica	1,250	None
Calliphora erythrocephala	1,760	None
Eristalis tenax	2,460	None
Aedes aegypti	1,260	Positive but very small
Ophion sp.	930	Positive
Trichocera sp.	480	Positive
Tipula sp.	850	Positive
Pieris brassicae	360	Positive
Nemeritis canescens	590	Positive
Drosophila sp.	370	Positive

10–15°C increase in frequency for decrease in air pressure from 1 to 0.1 atm.

For all insects showing a positive correlation between air pressure and wing frequency, $r_0 d$ is substantially smaller than the value for those showing no effect.

We write eq. (4) as follows.

$$I_w + I_{int} = K' b_0 r^2 \nu^{-2}, \qquad (8)$$

where $K' = K/4\pi^2$. For any given insect, I_{int} and $K' b_0 r^2$ are constants. Hence, if one plots I_w as a function of ν^{-2}, a straight line should result, with an intercept at $I_w = 0$ corresponding to the characteristic frequency of the muscle system alone, with the slope $K' b_0 r^2$. I_{int} can be extracted from the values of intercept and slope. A typical plot is shown in figure 1, based on Sotavalta's data for *Apis mellifica*. The results of this interpretation for all insects studied by Sotavalta (1952 and 1954) are given in table 1.

Roeder (1951) has classified insects as "synchronous" or "asynchronous," depending on the coincidence, or lack of it, between muscular spike potentials and wing beats. For such insects as *Periplaneta* and *Agrotis* he observes one spike potential per wing beat. For *Vespa* and *Calliphora*, on the other hand, each muscular spike potential is associated with from 5 to 20 wing beats. His data also appeared to show that for synchronous insects amputation of the wings had little effect on wing beat rate. Sotavalta (1954) performed systematic wing mutilations on a group of "synchronous" insects. He was kind enough to send me his original data, which

TABLE 1

Insect	ν (sec^{-1})	I_w (mg mm^2)	I_{int} (mg mm^2)	$K' b_0 r^2$ (10^{-3} erg)	M_t (mg)	M_w (mg)
Asynchronous insects—Sotavalta (1952)						
Apis mellifica	240	8.40	2.0	6.0	97	0.425
Theobaldia annulata	262	0.63	0.11	0.5	9.9	0.065
Bombus lapidarius	143	131	18.1	29.6	477	2.465
Bombus terrestris	156	158	19.8	40.3	880	2.860
Melolontha vulgaris	62	1,180	34	42.1	597	8.955
Eristalis tenax	175	15.9	3.0	5.65	50	0.655
Eristalis tenax	185	31.0	6.2	11.7	206	0.985
Calliphora erythrocephala	180	1.39	0.46	0.57	15	0.170
Calliphora erythrocephala	154	12.6	2.8	3.5	62	0.638
Calliphora erythrocephala	156	17.8	4.3	5.36	100	1.015
Tipula sp.	63	29.0	6.4	1.35	21	0.460
Tipula sp.	42	100	7.0	1.89	35	0.865
Tipula sp.	63	44.1	7.7	2.14	30	0.655
Tipula sp.	49	86.2	7.7	2.14	34	0.890
Tipula sp.	49	55.7	6.4	1.35	21	0.720
Tipula sp.	63	13.9	2.8	0.64	20	0.465
Tipula sp.	49	132	11.5	4.28	75	1.385
Tipula sp.	48	71.4	7.3	1.83	23	0.930
Tipula sp.	48	74.6	7.0	1.89	22	0.875
Tipula sp.	48	51.5	6.4	1.35	22	0.785
Vespa germanica	139	69.6	12.0	15.0	240	1.390
Vespa vulgaris	143	26.0	5.2	5.34	81	0.665
Trichocera sp.	67	0.674	0.082	0.033	1.57	0.050
Ophion luteus	62	32.2	4.8	1.32	33	0.675
Cerambycidae sp.	80	68.8	9.1	5.1	142	1.845
Pieris brassicae	10.5	4,230	2,010	7.30	144	17.975
Synchronous insects—Sotavalta (1954)						
Agrotis ypsilon	51	498	234	16.7	169	6.27
Amphitrota clandestina	45	541	508	20.3	150	6.29
Amphitrota clandestina	48	701	582	29.1	—	7.83
Sideridis unipuncta	41	548	109	11.5	—	6.88
Sideridis unipuncta	46	233	292	10.8	111	3.03
Amathes bicolorago	53	111	123	6.17	82	2.30
Sympetrum danae	41	640	603	21.0	90	4.4
Poecilocampa populi	55	265	252	14.8	112	5.23

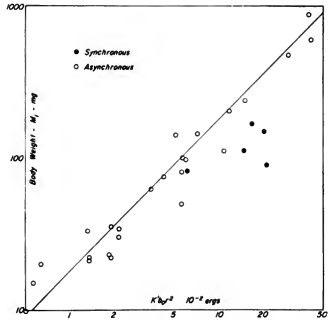

FIG. 2. Muscular restoring force and body weight. Data from table 1. The point for *Trichocera* sp. is not plotted. It is anomalous, perhaps because of the uncertainty, for so tiny an insect, in the measurement of weights of body and wings.

I have treated using the method described above. The data for the synchronous group are much less self-consistent than those for Sotavalta's (1952) asynchronous series. However, they seem in general to show that, from the point of view of oscillator theory, there is little difference between the two types.

The slope $K'b_0r^2$ should be related to some other characteristic of the insect, most probably to its weight. For if r is proportional to the wing length l, and l to b_0, then $K'b_0r^2$ is proportional to the weight of the wing muscle, M_m. It is reasonable to expect that M_m is proportional to the weight of the insect, M_t. In figure 2, the slopes $K'b_0r^2$ are plotted against the corresponding insect weights given by Sotavalta (1952 and 1954). It is apparent that at least for the asynchronous group the correlation is good even though the insects studied cover a wide variety of genera with weights varying from ten to nearly 1000 milligrams.

From figure 3, it appears that the internal moment of inertia, I_{int}, is related to the weight of the wing, M_w. I_{int} is clearly not related to the weight of the muscle M_m, since there is no close correlation between I_{int} and $K'b_0r^2$. Doubtless some portion of the internal and external insect skeleton oscillates with the muscle, and for some not readily apparent reason the inertia of this entire internal structure appears to be

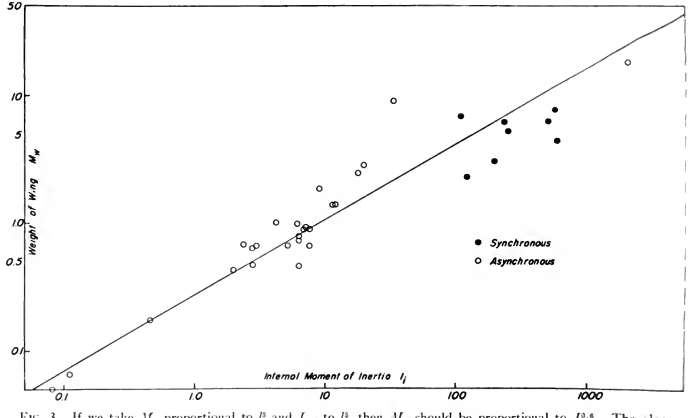

FIG. 3. If we take M_w proportional to l^3 and I_{int} to l^5, then M_w should be proportional to $I^{0.6}$. The slope of the line in the figure assumes the relationship.

ore nearly related to the weight of the wing of e insect than to its body weight.

Perhaps one caveat should be introduced re—that the resonance frequency can be aintained only so long as the damping or ading does not exceed the driving capacity of e muscle. This condition is likely to obtain ile the insect or bird leads its natural life. hen wing loads are applied artificially, such was done for insects by Sotavalta (1952) and hers, the damping may be too large to be overme by the muscle drive. The amplitude, Φ, the driven system, eq. (1), under steady-state nditions is given by eq. (6). As B increases, Φ n be held at its appropriate value for a given ght regime and ω at ω_0 for maximum economy energy consumption, so long as muscle chemry can increase F_0 correspondingly. However, ien F_0 can no longer keep pace with increasing as may happen when artificial loads are aped, Φ and also ω may change to the point that e flight becomes impossible.

When aerodynamic requirements for steady ght are such that $B\dot{\varphi}$ decreases, maximum onomy dictates a decrease in the amplitude Φ. ie observes this condition for large birds in ising flight. Pelicans, for example, flying ose to the water appear almost to glide, i.e., e amplitude Φ decreases to a very small value. ould they change course or go into a rapid mb, the amplitude increases markedly. The ng frequency, however, remains constant.

One frequently observes an increase in wing equency for large birds under unusual and enuous flight conditions; a female tropic bird, example, braking before landing at her nest- burrow or eluding pursuit by a demanding ale. For this increase in aerodynamic energy tput, F_0 must increase inordinately, since we ve not only the increased energy output, but so the superimposed losses due to departure om resonance frequency. Limited observaons would indicate that such conditions can maintained only for brief intervals.

The motion of the wings of "ordinary" birds more complex than is the case for insects or mmingbirds. For the former the wing is lly extended only during the down beat; ring much of the up beat it is folded more or is closely in to the body. Therefore, the oment of inertia is less for the up beat than for e down beat and one would expect a correonding decrease in the duration of the up beat. or the chickadee, high-speed motion pictures show this to be the case—the time interval for the down beat is 50 per cent greater than for the up beat. This implies a departure from the simple harmonic motion assumed for the wing beat of insects and hummingbirds. The solutions of eq. (7) for such birds would require knowledge of the time dependence of inertia and damping throughout the entire wing beat for each species considered. Sufficient data are not available even for an approximate solution. The basic principles implicit in the simpler equations would, however, still be valid. The wings of ordinary birds will beat at a resonance frequency, and the consequences of a departure from the resonance condition will be qualitatively similar to the situation for insects and hummingbirds.

It is interesting also to note that the dependence of wing beat rates on the size of dimensionally similar groups of flyers is consistent with eq. (4). Sotavalta in several papers (1947, 1952, 1954) gives wing frequencies and wing lengths for a host of insect species. I have determined wing frequencies and wing lengths for perhaps forty hummingbird species ranging from *Calliphlox amethystina* (male) ($\nu = 80$ sec^{-1}) to *Patagona gigas* ($\nu \simeq 10$ sec^{-1}), and also for a number of passerines ranging in size from chickadee to mockingbird. Meinertzhagen (1955) gives data for a number of large birds whose wing frequencies were sufficiently low to permit visual counting. The literature, unfortunately, gives very little additional data on wing frequencies of birds, particularly those of small or intermediate size.

Figure 4 shows all of these data plotted against wing lengths in logarithmic coordinates. One sees that the points appear to be bounded by a straight line described by the empirical relation

$$\nu l^{1.15} = \text{constant}. \tag{9}$$

In figure 4, wing length is the distance from wing tip to the first articulated joint. For insects and hummingbirds, this is the whole length of the wing since the first articulated joint is at the shoulder. For birds, the first joint is at the wrist, and the length as given is about 60 per cent of the length of the fully extended wing. The measurements for birds join smoothly with those for insects, presumably because the length from tip to wrist for ordinary birds happens to be roughly equal to the effective average wing length during flight.

The detailed relationships between l, b_0, r,

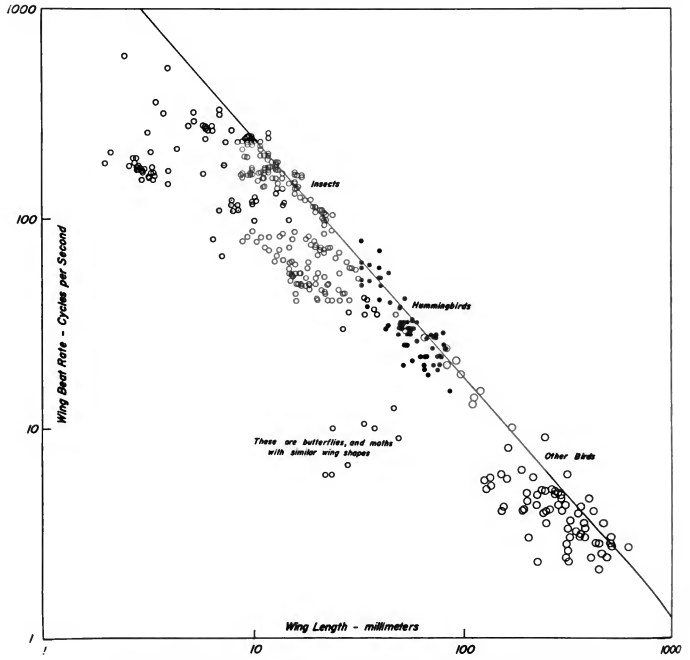

FIG. 4. Wing beat rate and wing length for insects, hummingbirds, and other birds. The equation of the boundary line is $\nu l^{1.15} = 3540$.

and I depend on the anatomy of a particular family of birds or insects. Yet one would expect that b and r are roughly proportional to l, I proportional to l^3, and M_i to l^3. For such a series of animals eq. (4) predicts

$$\nu l = \text{constant}. \tag{10}$$

For insects, such relationships appear to hold. When we group Sotavalta's data into insect families which are more or less dimensionally similar, we find groups of straight lines which agree with eq. (10), each line terminating approximately at the boundary line given by eq. (9).

For hummingbirds the anatomical relationships appear quite different. Here we find the weight of the bird proportional to $l^{1.5}$, not l^3. Taking r proportional to l, and b_0 to $M^{1/3}$, hence to $l^{0.5}$, we find $\nu l^{1.25} = \text{constant}$.

For birds, A. Magnan (1922) gives data on wing length, wing area, and wing weight for several hundred species whose body weight ranges from 4 grams to nearly 10 kilograms. Sotavalta gives similar data for perhaps sixty

insect species whose body weights range down to a few milligrams. When wing lengths and wing areas for this array of flying animals are plotted in logarithmic coordinates one finds a slope of 2, indicating, as would be expected, that wing area is proportional to l^2. However, when wing weights are plotted against wing area, the data fall on a continuous straight line with slope 1.65—not 1.5, as might have been expected. This implies a variation in wing thickness with $l^{1.3}$, in accord with the empirical boundary relation of eq. (9). Perhaps Nature increases the relative thickness of the wing to limit flexing at the wing tip under conditions of normal flight.

In any event the boundary relation per se is not founded in oscillator theory but is in all probability related to aerodynamic efficiency with Nature making small changes in her "model" as the flying animal increases in size to conform to sound and efficient principles of avian design.

It is unfortunate that the ornithological literature is so devoid of wing frequency measurements. It is to be hoped that another Magnan will emerge who will fill this gap in our knowledge of birds as thoroughly and as convincingly as Sotavalta has performed the task for insects.

In summary, the wing beat rates of all insects and hummingbirds studied, and probably of most birds, can be fully explained by the theory of mechanical oscillators. Within dimensionally similar families the frequencies vary with the size of flyer, in accordance with the equation νl^n = constant, as expected from this theory. The value of n depends on the dimensional relationships for the family, and appears to fall within the limits 1.0 to 1.25. For all birds and insects there appears to be a limiting boundary for which $\nu l^{1.15} \simeq 3540$, where ν is full wing beats per second and l is in millimeters.

There is, of course, no inherent relationship between wing frequencies, as regulated by oscillator theory, and flight techniques or flight efficiencies. Oscillator theory per se relates only to wing beat rate and gives no clues as to how the beating wings produce lift or propulsion.

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THE VOYAGE OF JEAN RICHER TO ACADIA IN 1670: A STUDY IN THE RELATIONS OF SCIENCE AND NAVIGATION UNDER COLBERT

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It hardly seems plausible that a single obscure voyage should epitomize and illustrate most of the relevant aspects and interrelations of French science and navigation in the decades following the accession of Louis XIV in 1660. Yet this is what a little-known voyage to the coast of New England and Acadia in 1670 does, both for France generally, and for the Académie Royale des Sciences, which had been established in Paris in 1666 under the protection of Louis' great minister, Jean-Baptiste Colbert (1619–1683).

Until recently, the accomplishments of the Academy during its early years have characteristically been underestimated or misunderstood. This has been equally true of French science as a whole during the half-century after 1660.[1] Similarly, the decisive importance of the contemporary French contribution to the establishment of scientific cartography has been slow to gain recognition.[2] It is thus not surprising that the related efforts to improve navigation are inadequately known or appreciated. Indeed, the climate of historical opinion has tended, ever since the early eighteenth century, to be unfavorable to a fair evaluation of French work in both the latter areas, just as it has to French science in general. In the case of the Academy this has made for a serious underestimation of the importance of its early work, and an overestimation of that of its rival, the Royal Society of London (1662).

A study of the voyage which one of the Academy's *élèves astronomes*, or assistants in astronomy, Jean Richer (1630–1696), made to the North American coast in 1670 will not automatically set these matters right. Yet such an investigation, embracing not only the voyage but its antecedents as well, may help to place contemporary French science, cartography, and navigation in a more favorable light. In doing so it will not only clarify the role and contribution of the Academy of Sciences, but also suggest the character and scope of the contemporary relationships of science—more specifically astronomy—and navigation.

These interrelations are most clearly revealed in attempts to solve the problem of longitude, not the least of which is the problem of longitude at sea. It is therefore intriguing as well as somewhat ironic that the voyage across the Atlantic to Acadia was more than likely intended to provide concurrent tests of the practicability at this period of *both* the methods by which the riddle of longitude at sea was ultimately solved a century later. Actually, as we shall see, the attempt was premature. For although some of the requisite astronomical tables, optical instruments, and timekeepers existed in 1670, none were as yet adequately refined to make the success of either method a practical possibility. The conditions essential to such success, were, nevertheless, clearly understood.

Another point of interest about the Acadian voyage is that it was the occasion for some careful observations of the height of the tides on both sides of the Atlantic as well as for the determination of the latitude of two points on the North American coast. The latter observations appear to be the earliest in this region made by an experienced observer employing instruments of the best contemporary quality.[3]

[1] The historiographical basis for this situation is of some interest and might warrant investigation.

[2] The nature and extent of the French contribution were shown conclusively by C. Sandler, in *Die Reformation der Kartographie um 1700* (Munich and Berlin, Oldenbourg, 1905), early in the present century. The important study of L. Gallois, L'Académie des Sciences et les origines de la carte de Cassini, appeared soon after in *Annales de Géographie* **18**: 193–204, 289–310, 1909. The slowness with which this information has made its way into the relevant literature in English is surprising—in any detailed way hardly prior to the two works by L. A. Brown: *Jean Dominique Cassini and his world map of 1696*, Ann Arbor, University of Michigan, 1941; *The story of maps*, Boston, Little, Brown, 1949. The latter work includes a categorical recognition of the dominantly French creation of scientific cartography late in the seventeenth century.

[3] No direct information about the instruments Richer actually took to Acadia has been found. Those at his disposition at La Rochelle were certainly representative of the best then available in France, and hence among the most advanced to be had. Presumably the instruments were numerous. An indication of Richer's

Finally, the voyage constitutes an unrecognized tance of a visit to the North American main- d by men attached to the Royal Academy of ences. Although members and associates of s body were to travel widely in the course of Academy's development and application of the dern scientific expedition—a significant device extending the range and effectiveness of ob- vation and experiment—few among them ever l any direct contacts with North America.[4] e very singularity of the visit of 1670 might in lf justify the investigation of the history and ecedents of a voyage the traces of which time largely obliterated.[5]

I

t is a truism that the late fifteenth century rked a new era in the history of navigation. e rapid expansion of oceanic voyaging both ed new problems and rendered old ones acute. re and more two basic conditions for safe and cient navigation on the high seas, neither easy fulfill, were recognized. One was the navi- or's need to be able to determine with pre- on his position on the surface of the ocean. e second condition was accurate maps to which refer this position. The progress of cartogra- and navigation had thus to go hand in hand. Because the basic controls were in both in- ces astronomical, the improvement of maps as l as of navigation hinged on the development accurate means for determining longitude as l as latitude. The latter presented no great blem: even on shipboard fair results had for turies been obtained with such instruments as astrolabe or cross-staff for taking the altitude the sun by day or of Polaris by night. The nement or replacement of the instruments and ronomical tables currently in use was in this case all that was required.[6] Longitude posed a more difficult problem.

Even for cartography, dependable knowledge of differences in longitude hardly existed in 1670. The plight of navigation was far worse: anything beyond the approximation of a ship's longitude yielded by dead-reckoning (often surprisingly good in the hands of experienced navigators) was lacking altogether. Nor would this situation be ameliorated for another hundred years. Only when the two alternative methods of "lunar distances," and of differences in local time by means of accurate marine chronometers, had been perfected during the second half of the eighteenth century would the problem of longitude at sea, after centuries of effort, be solved.

Actually, the feasibility of both methods had long been recognized. Hipparchus, the Greek astronomer, at least understood the use of differences in local time two centuries before the Christian era. He had to rely, however, on information gained from simultaneous observations in two or more places of eclipses of the sun or moon, rather than on information supplied by the transportation of a mechanical timekeeper from one place to another. The method of "lunar distances," which involves the measurement of the angular distance from the moon to the sun or to certain fixed stars (accurate tables of the movements of the moon were, of course, presumed), was understood well before 1600.[7]

Late in the seventeenth century an almost revolutionary increase in the accuracy of maps took place. Its basis was the successful determination of longitude by quite another method, a method practicable, as was quickly recognized, only on land. This method involved the determination of differences in local time by means of simultaneous observations of the eclipses of one or another of the four major satellites of Jupiter. As the work of Italian and French astronomers after 1668 was to demonstrate, these were actually

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aration for the accurate observation of latitudes is . Wolf, *Histoire de l'Observatoire de Paris de sa ndation à 1793*, 10-11, Paris, Gauthier-Villars, 1902. temporary interest in the tides was great and widely enced.

On the origins of the modern scientific expedition, see W. Olmsted, The expedition of Jean Richer to enne (1672-1673), *Isis* **34**: 117-118, 126-128, 1942.

The sources for an early, relatively obscure voyage ld tend to be scanty. What specially complicates reconstruction of the voyage in question is the loss he manuscript minutes of the Académie des Sciences 1670-1674, together with the bulk of Richer's corre- ndence and reports relating to the voyage. Nor has log of the ship on which Richer sailed been found.

[6] On the general problem of latitude, *cf.* F. Marguet, *Histoire générale de la navigation du XV^e^ au XX^e^ siècle*, 104-126, Paris, Société d'éditions géographiques, maritimes et coloniales, 1931; L. A. Brown, *Story of maps*, 180-207.

[7] On the question of longtitude, *cf.* F. Marguet, *op. cit.*, 127-260; L. A. Brown, *Story of maps*, 208-240. A useful recent work on early navigation is E. R. G. Taylor, *The haven-finding art*, London, Hollis, 1956. The standard work of R. T. Gould, *The marine chronometer, its history and development*, London, J. D. Potter, 1923 deals primarily with the post-seventeenth-century period in England.

the only eclipses well suited to the reliable determination of longitude.[8] In the case of lunar and solar eclipses, as well as of the "occultation" or eclipse of a star or planet by the moon, the instant of contact or of immersion in the shadow or emersion from it could not be observed with sufficient accuracy. Moreover, the relative infrequency of lunar and solar eclipses, as well as of occultations, made them less useful than eclipses of the satellites of Jupiter.

The possibility of using the eclipses of the satellites for the determination of longitude had been recognized early in the century concurrently with Galileo's discovery of these "moons" of Jupiter.[9] But it was two generations before relatively accurate ephemerides of the movements of the satellites, published in 1668 by an Italian astronomer, Giovanni Domenico Cassini (1625–1712), made the method practical.[10] Cassini's call to Paris in 1669 to become a resident member of the new Academy of Sciences was not unconnected with this development. His persistent efforts to apply the method were well seconded by those of his colleagues who were astronomers.

In this way, the magnificent new *Observatoire de Paris,* where members of the Academy observed, came to serve as the center of what might appropriately be termed a "bureau" of longitude and cartography. Here observations of the eclipses were arranged and carried on, the cooperation of astronomers in other countries solicited, missionaries with overseas assignments given training in the techniques of observation, and the observations subsequently obtained recorded and entered on a special world map.[11] From the 1670's and 1680's on, an increasing number of observations began to flow in. The results were startling: the longitude of key points in the Far East in error by from 20° to 27°; the length of the Mediterranean seriously overestimated on virtually all maps; the Atlantic coast of France too far to the west by nearly 100 miles; and so on.[12] By 1700, as a result of these activities, a virtual revolution in cartography—a revolution accomplished in France, it should be emphasized—had taken place.

In bringing about this advance, the pendulum clocks developed by the great Dutch scientist, Christiaan Huygens (1629–1695), had contributed notably. As early as 1666, when Huygens was brought to Paris in anticipation of the establishment of the Academy of Sciences, his clocks, after a decade of tinkering and refinement, were capable of a high degree of accuracy.[13] The alert

[8] On the method and its application, *cf.* G. Bigourdan, *L'astronomie. Evolution des idées et des méthodes,* 168–170, Paris, E. Flammarion, 1911; F. Marguet, *op. cit.,* 127–131; L. A. Brown, *Story of maps,* 215–223, based on the fuller discussion in his *Jean Dominique Cassini and his world map of 1696.*

[9] For the first attempts to apply the method to the improvement of cartography and navigation by means of concerted observations, see accounts of the activities of what has been termed a "bureau of longitude" at Aix, in Provence, *circa* 1633 to 1636, centering around the amateur, Nicolas-Fabri de Peiresc, and the philosopher-astronomer, Pierre Gassendi, in P. Humbert, *Un amateur: Peiresc, 1580–1637,* 211–237, Paris, Desclée, de Brouwer, n.d. [1933]; G. Bigourdan, *Histoire de l'astronomie et des observatoires en France* 1: 32–43, 2 pts., Paris, Gauthier-Villars, 1918–1930. The most recent account is Seymour L. Chapin, The astronomical activities of Nicolas Claude Fabri de Peiresc, *Isis* **48**: 23–27, 1957. Whether, as Chapin contends, some of these activities amounted to genuine scientific expeditions of a new type is a point which wants clarification.

[10] *Ephemerides Bononienses mediceorum syderum, ex hypothesibus et tabulis Joan. Domin. Cassini,* Bologna, 1668.

[11] L. A. Brown, *Jean Dominique Cassini and his world map of 1696, passim* (to be used with caution); C. Sandler, *op. cit., passim.* The standard work on the construction and history of the *Observatoire de Paris* is by C. Wolf, as cited in n. 3, above.

[12] See the works cited in note 11. Observations of the lunar eclipse of 28 August, 1635, had led Peiresc to the conclusion that contemporary maps of the Mediterranean based on Ptolemy's longitudes, were highly inaccurate as regards its length. According to Chapin, in the article cited in n. 9 above, the conclusion reported by Peiresc in his correspondence is that the accepted length was excessive by 200 to 300 leagues, i.e., 600 to 900 miles. *Isis* **48**: 25, 1957.

[13] Huygens' correspondence, papers, and published writings, especially the *Horologium* (1658), and *Horologium oscillatorium* . . . (1673), constitute the fundamental sources for these developments. All are now available in the great collection, *Œuvres complètes de Christiaan Huygens publiées par la Société hollandaise des Sciences,* 22 v., La Haye, M. Nijhoff, 1888–1950, cited hereafter as *Œuvres complètes.* See esp. vols. **2–8, 17–18, 22,** *passim.* A reliable short biography of Huygens in English is A. E. Bell, *Christian Huygens and the development of science in the seventeenth century,* New York, Longmans, Green, 1948. Brief suggestions regarding the invention, use, and success of Huygens' astronomical clocks occur in L. A. Brown, *Story of maps,* 211 ff; R. T. Gould, *The marine chronometer* . . . 27–30. For more extended treatment, see L. Defossez *Les savants du XVII^e siècle et la mesure du temps passim,* Lausanne, Edition du Journal suisse de horlogerie et de bijouterie, 1946, and J. D. Robertson, *The evolution of clockwork,* London, Cassell, n. d. [1931], esp. ch. 6, 9. Though dealing less with theoretical, scientific elements, the latter work is more thorough, and based on considerable use of original documents.

ronomers of the Academy, men like Adrien zout (1622–1691), and the Abbé Jean Picard 520–1682), were quick to seize on the clocks as e means of improving the accuracy of a wide ige of astronomical observations. An even re pressing application in the eyes of those ncerned with the improvement of navigation s the use of clocks on shipboard for the de- mination of longitude. Had this proved feasi- , the application would, of course, have antici- ed by nearly a hundred years the success erved for the marine chronometers perfected England and in France in the course of the ond half of the eighteenth century.

II

During 1667 and 1668, members of the new ademy were busily planning an important pro- am of astronomical observations. To carry out ose which needed to be made overseas, pro- sals to send observers to such places as adagascar were under consideration.[14] More- er, it so happened that the Academy's sponsor, olbert, and its leading member, Huygens, had ig been interested in the problem of determining ngitude at sea. As a result, from 1667 until ·ll after 1670, numerous attempts to find a lution to this baffling question were made in rance, both inside and outside the Academy. Huygens' interest in the problem arose directly om his invention of the pendulum clock. Within fortnight of the completion of his first model December, 1656, he raised the question of the ssible service of such a clock to navigation. wo years later, when his recently published eatise, the *Horologium*, was bringing detailed nowledge of the new clock to the learned world, and a Scottish collaborator, Alexander Bruce, arl of Kincardine, who had been in exile in the etherlands, tried their hands at a design suit- ble for use at sea. In 1662 this first marine ock, employing a short pendulum, was com- eted and given preliminary tests preparatory to trial on shipboard.[15]

The first in a long series of such trials was made early in 1663 with clocks Huygens had had constructed for Bruce. The occasion was a voyage made by the latter across the North Sea to England. Weather conditions were quite unfavorable, however, and the trial was unsuccessful. Bruce and his colleague of the Royal Society of London, who had been informed of what was going forward, were not discouraged. During the course of 1663 they managed to have the clocks tested on board the ship of Captain (later Admiral) Sir Robert Holmes of the Royal Navy on a voyage to Lisbon and return.[16] This was followed late in 1663 and 1664 by an even more exacting trial. Again the clocks were sent with Holmes, who was in command of a small naval squadron destined for the Guinea coast and, so Huygens was informed, Jamaica.[17]

The principal political result of this voyage, chiefly through Holmes' high-handed seizure of Dutch trading stations in Africa, was the acceleration of the outbreak in 1665 of the second of England's seventeenth-century commercial and colonial conflicts with the Netherlands. The scientific results of the voyage were entirely pacific and at first seemed quite promising.

The apparent success of the marine clocks during this extended trial, while not conclusive, greatly encouraged Huygens.[18] Previously he had been rather pessimistic about the outlook because of inequalities in the daily rates of going

[14] Olmsted, *Isis* **34**: 118–119, 1942. An early outline a general astronomical program for the Academy, in uygens' handwriting, and probably dating from 1666, in *Œuvres complètes* **19**: 255–257.

[15] *Ibid.* **2**: 5, 109; **4**: 68, 72, 151. For details of the arine clocks, 1662–1670, see *ibid.*, **17**: 164–182; **18**: 24; **22**: 174, 579–582, 586, 593–594, 604–608. Numer- is original sketches are reproduced. A tentative *mise- point*, based largely on the *Œuvres complètes*, is in efossez, *op. cit.*, 173–178.

[16] *Œuvres complètes*, **4**: 274–275, 278, 281, 284–285, 287–288, 290–291, 296, 304, 306, 318, 426–428, 431–432, 443–451, 458; **17**: 166–167, 193. According to Holmes' journal, the period the clocks were observed was 28 April-4 September, 1663.

[17] *Ibid.*, **4**: 428, 443. Possibly Moray's statement of 29 November, 1663, that the clocks would be sent to Guinea and thence to Jamaica was made in good faith. The secret object of Holmes' voyage, however, was limited to support of English trade and traders in Africa against the Dutch. The widely disseminated statement that it was Holmes' squadron which, presumably after crossing the Atlantic from Africa, captured New Amsterdam from the Dutch in August, 1664, is false, as Holmes' journal proves. See J. C. M. Warnsinck, The legend of Holmes at New York, *Mariner's Mirrour* **22**: 238–239, 1936.

[18] *Œuvres complètes* **4**: 432, 444; **5**: 284–285; **17**: 193–194, 230–234; **22**: 174. On the origins of the Second Dutch War, 1665-1667, the fundamental discussion is H. L. Schoolcraft, The capture of New Amsterdam, *Engl. Hist. Rev.* **22**: 674–693, 1907, which first showed the primary importance of Anglo-Dutch conflicts of interest in West Africa and the contribution to open conflict made by Holmes' seizure of Dutch trading posts on the Guinea coast.

of the clocks both ashore and afloat. Now his confidence in ultimate success returned. Plans were considered for the manufacture and sale of the marine clocks under conditions which would secure his financial interests.[19]

In Paris, meanwhile, knowledge of what Huygens and his English collaborators were attempting had become quite widespread. Indeed, the Dutch scientist himself early informed one of Colbert's influential advisers, the poet Jean Chapelain, of his hopes for the clocks. Subsequently he reported the progress being made. The enthusiasm evinced by Huygens about the performance of the clocks during Holmes' voyage to Africa seems to have determined Chapelain to see that some of their potential advantages were secured for France. By a variety of means, he strove to arouse public interest in the clocks as well as to have their inventor brought to France. In June, 1665, at a time when interest in the new marine clocks was growing in scientific and official circles, negotiations with Huygens were begun.[20]

During the same period, pendulum clocks which Huygens had had made in Holland for the Parisian amateurs, Carcavy and Montmor, were ready for delivery. One was of the marine type. Writing of it to Carcavy on August 20, Huygens noted: "This . . . [clock] will be of no little utility to your Company of the [East] Indies once the use of it has been begun. . . ." And, he added, having apparently come to a vital decision of his own, "this is what I shall work at as soon as I have come to France."[21] In a competition which was to prove important to science as well as to navigation, it appeared that the French had won the first round from their English rivals.

III

The creation in 1664 of a notable overseas trading company, the Compagnie des Indes Orientales, marked the realization of a favorite project of Colbert for the strengthening of French commerce in the region of Madagascar and the Indian Ocean. Huygens' opinion of the potential value of his marine pendulum clocks to the navigations of the Company was not likely to pass unnoticed by Colbert, whose interest in navigation and cartography, as previously indicated, was of long standing. Indeed, Colbert's formation of the Academy of Sciences in 1665–1666 was, in part, a response to the pressing needs of French navigation for the improvement of maps, the development of hydrography, and the solution of the problem of longitude. In this and other ways, Colbert gave an impulsion to nautical science in France which was to be felt long after his death in 1683.[22]

Initially it appears to have been the intention to test Huygens' clocks on a voyage to Madagascar, the East India Company's overseas headquarters during its early years. Thus in 1666, in the course of discussions among members of the nascent Academy about sending an observer to the island in the interests of astronomy, the opportunity this voyage might afford for testing the marine clocks was not overlooked. When, barely three weeks after the first meeting of the Academy (December 22, 1666), more precise plans for the scientific expedition to Madagascar were presented to the members by Adrien Auzout, the trial of Huygens' clocks was specifically recommended. It was perhaps no coincidence that Huygens, having established himself in Paris during the course of the previous year, was at the time supervising the construction of three marine-

[19] *Œuvres complètes* **2–6**: *passim*, contain much correspondence on the latter subject. On the specific steps taken in Holland, England and France, see esp. **17**: 175–177, and notes; **18**: 7–9, 20, and notes.

[20] H. L. Brugmans, *Le séjour de Christian Huygens à Paris et ses rélations avec les milieux scientifiques français . . .*, 37–39, 50, 56, 59–60, Paris, Impressions P. André, 1935; A. J. George, A seventeenth century amateur of science: Jean Chapelain, *Annals of science* **3**: 222–231, 1938; *Œuvres complètes* **2**: 166, 181, 266; **5**: 110–112, 204–205, 222–223, 375, 397. French interest in Huygens at this time was in no sense narrowly utilitarian. The fact that his work on Saturn aroused quite as much curiosity and emulation as did his marine clocks is worth remarking.

[21] *Ibid.* **5**: 438, 439–440; **17**: 8–9. The importance at this period of Carcavy's position and influence *vis-à-vis* Colbert is shown effectively in the unpublished doctoral dissertation of John Milton Hirschfield, The Académie royale des sciences (1666–1683): inauguration and initial problems of method, University of Chicago, 1957. I am indebted to the author for an opportunity to examine this promising study.

[22] *Cf.* C. de la Roncière, *Histoire de la marine française* **5**: 410–417, 6 v., Paris, E. Plon-Nourrit, 1898–1932; Didier-Neuville, Les établissements scientifiques de l'ancienne marine, *Revue maritime et coloniale* **56**, **57**, **59**, 1878; **62**, 1879; **66**, 1880; P. Clément, ed., *Lettres, instructions, et mémoires de Colbert* **3**[1], **4**: *passim*, 8 v., Paris, 1861–1882. R. Mémain, *La marine de guerre sous Louis XIV*, Paris, Librairie Hachette, 1937, is also pertinent.

type clocks which might be used for this purpose.[23]

As yet, however, fortune had not smiled on French efforts to develop maritime and commercial relations with Madagascar. Indeed, the East India Company, struggling to exploit the island's resources, was having to fight for its very existence.[24] To a variety of adversities, those of war had been added. First in point of time and importance was the Second Dutch War (1665–1667) between England and the United Netherlands. This conflict, as already indicated, Captain Holmes' African expedition had helped to precipitate. France, an ally of the Netherlands since 1662, reluctantly declared war on England in January, 1666. Thereafter French maritime relations overseas became quite uncertain, those with Madagascar and the East Indies suffering in particular. Across the Atlantic, local French successes in the West Indies were subsequently offset by English naval action in that area, and by the loss of the colony of Acadia in the north.[25] The belated return of the latter to France in 1670, under the terms of the treaty of Breda of July 21, 1667, which ended the war, was to provide the occasion for the little-known voyage and the scientific observations with which this paper is extensively concerned.

Whether the War of Devolution against Spain, which followed in May of 1667, had any direct or indirect bearing on the delay in the proposed scientific expedition to Madagascar, the documents do not reveal. Possibly the wish to avoid complicating in any way the troubled affairs of the East India Company was, as has been suggested, a more important factor.[26] Or it may have been that the wartime preoccupations of Colbert, whose official approval of any expedition involving members of the Academy of Sciences had to be obtained, served to delay authorization of the voyage to Madagascar, and hence of the related testing of Huygens' clocks. In any case, the best that could be had in the circumstances was a more restricted trial of the clocks shortly before the Treaty of Aix-la-Chapelle of May 2, 1668, ended the war with Spain.

IV

In the course of 1668 and 1669, governmental and scientific groups in Paris were considerably occupied with the question of longitude. One reason was that several "discoveries" asserted to provide practical solutions to the stubborn problem were presented to Colbert during these years.[27] Among them was that of "Sieur André Reusner de Neystett, of the German nation, formerly colonel of a Swedish regiment." Neystett's proposal was referred to a special committee composed of members of the Academy of Sciences, plus a high-ranking naval officer, the famous Abraham du Quesne, and Colbert. Although recognizing some ingenuity in the "discovery," a kind of marine odometer to be installed in a ship's hull, the committee reported it quite unsuitable for use.[28] Fontenelle's often repeated statement that Neystett received 60,000 livres even before the trials took place is unsupported by Colbert's account books and hardly accords with the minister's well-established reputation for shrewdness.[29]

[23] A. J. George, The genesis of the Académie des Sciences, *Annals of Science* **3**: 385, 1938; Olmsted, *Isis* **34**: 118–119, 122 and note 53, 1942; Huygens, *Œuvres complètes* **19**: 256; **18**: 9; **6**: 129. Notes 7, 8, 9 of the last reference are in error and should be disregarded.

[24] Charles W. Cole, *Colbert and a century of French mercantilism* **1**: 504–509, 2 v., New York, Columbia University, 1939. I owe this reference and the suggestion it supports to Mr. Hirschfield's dissertation, referred to in n. 21, above.

[25] These and subsequent events are discussed in the standard histories of such authors as P. J. Blok, G. N. Clark, E. Lavisse, and D. Ogg. The best general account of French relations with Madagascar is in J. Sottas, *Histoire de la Compagnie royale des Indes Orientales, 1664–1719*, Paris, Plon-Nourrit, 1905.

[26] John M. Hirschfield in the dissertation cited in n. 21, above.

[27] The documents disclose the names of five men either interested in or who put forward various "discoveries" during this period, viz., Van Gangel, Reusner de Neystett, Jacques Graindorge, Nicolas Mercator, and Desbayes, the latter being directly involved in the Acadian voyage of 1670. On the first four, the most convenient source is *Œuvres complètes* **6**: 171–172, 200, 378–379.

[28] The full account is in the manuscript *procès-verbaux* of the Academy, Registres de l'Académie des Sciences (cited hereafter as Acad. Sci., Registres) **2**: 38–53, 1668–1669. It is summarized in part in *Œuvres complètes* **6**: 378, n. 1. The Registres reveal strikingly the extensive discussions of problems relating to longitude and navigation in the meetings of the Academy during this period.

[29] L. A. Brown, *Story of maps*, 214–215, is a recent writer who relies on Fontenelle's somewhat ambiguous account, *Mémoires de l'Académie royale des Sciences depuis 1666 jusqu'à 1699* **1**: 45–46, 11 v. in 14, Paris, 1729–1733. (The paging of the various printings of this set, abbreviated hereafter as *Mém. Acad. Sci., 1666–1699*, varies considerably.) This is Brown's authority for the assertion that Neystett was given a patent (*brevet*) on his invention, "sight unseen," and paid 60,000 livres in cash. The Academy's minutes, cited in n. 28, are free of any ambiguity: such a payment to Neystett,

For their own part, members of the Academy appear to have been pretty well convinced that one of the best chances of solving the problem of longitude at sea lay in the further improvement of the marine clocks. Thus in March, 1668, probably at Huygens' initiative, one of the Academy's assistants, a M. Delavoye, was sent to test two of Huygens' clocks on board the flagship of an admiral of the French fleet in the Atlantic, M. de Beaufort. When, on March 29, with hostilities against Spain still in progress, Beaufort sailed from Brest with a small squadron for an attack on Corunna, Delavoye, together with his clocks, astronomical instruments, and written instructions prepared by Huygens, was on board the *Saint-Phillipe*.[30]

This time Huygens' hopes for success were high. Heartening news reached him as early as May 11. During a great tempest encountered by the fleet sometime in April, the clocks had not once stopped because of the storm. Writing to Colbert on June 22, a few weeks before the return to Brest, Delavoye expressed confidence in the complete success of the clocks, which in his opinion were destined to prove of great utility both to navigation and in the construction of improved marine and "terrestrial" maps.[31]

In the end, however, careful examination of the records of the voyage produced a different impression. The results, Huygens was forced to admit, were quite unsatisfactory. Mechanical faults in the clocks were partly to blame, he concluded. But, he alleged, Delavoye had handled the clocks badly and thus contributed to their failure. For a time Delavoye was under a cloud.[32]

Huygens was not seriously discouraged. To protect his invention against encroachment, he pleaded with Colbert for a trial of the mechanically improved clocks by a genuinely competent person on a long oceanic voyage. This plea was only partially successful. What was authorized was not an extended voyage in the Atlantic, but rather one in the Mediterranean from Toulon to Crete with a French expeditionary force under Beaufort for raising the twenty-year-old Turkish siege of the Venetian stronghold of Candia. Moreover, custody of the single clock employed was confided to Delavoye. Having been reinstated in Colbert's good graces, he was apparently on board Beaufort's flagship, *Le Monarque*, when the expedition weighed anchor on or about June 5, 1669. At the end of September—possibly a little later—Delavoye returned to Toulon.[33]

The results of this second voyage seemed encouragingly good. The clock had stood up well against the vibration caused by the ship's gunfire; even under the shock of the explosion of a near-by vessel, the *Sainte-Catherine*, it had not stopped. Further ground for Huygens' optimism was provided by the determination of the difference in longitude from Candia to Toulon with an apparent error of not over five or six leagues. Such an error was normally made by pilots in their reckoning of position after two days' sailing. Yet, on the return voyage, Delavoye's ship had been at sea from 22 July to 29 September without sight of land. Everything hinged, of course, on the accuracy of Delavoye's observations

or the issuance to him of letters-patent, is conditional upon the successful demonstration of his device before the royal commissioners. This is the import of the less explicit statement made by the contemporary secretary and historian of the Academy, J. B. Duhamel, *Regiae scientiarum Academiae historia*, 42–44, 2nd ed., Paris, 1701. The only payment to Neystett recorded in the official accounts of Colbert's office, *Comptes des bâtiments du roi sous le règne de Louis XIV*, ed. J. Guiffrey, 1: 279, 5 v., Paris, 1881–1901, is dated 27 August, 1668, about three months *after* the rejection of the invention. The entry reads: "To M. Reusnier [*sic*] for having come here from Germany and Holland bringing various machines . . . 3000 [livres]"—a considerable sum for that day, and indirect evidence of the high valuation placed by Colbert on the solution of the problem of longitude.

[30] Auzout to Oldenburg, 17 March, 1668, Royal Society of London, Guard Books, A, 21; *Œuvres complètes* 6: 200; La Roncière, *op. cit.* 5: 474–475; G. P. Depping ed., *Correspondence administrative sous le règne de Louis XIV* 4: 567, 4 v., Paris, 1850–1855. Delavoye (? – 1684), sometimes referred to as La Voye-Mignot, and described as an astronomer and engineer, was named an *élève* of the Academy of Sciences in 1666. Although details of his first voyage with Beaufort are scarce, it need not be confused with that to Candia as is done by Huygens' editors, *Œuvres complètes* 6: 187, n. 8; 281, n. 12. The instructions prepared by Huygens were presumably those of 1665 first issued in Dutch, and later published in Royal Society, *Philosophical Transactions*, 10 May, 1669. These instructions are reprinted in *Œuvres complètes* 17.

[31] *Œuvres complètes* 6: 218, 226. Delavoye's letter written from "Vignes" (Vigo?), is known only from the copy in Huygens' hand printed in this work.

[32] *Ibid.* 6: 379; Depping, *op. cit.* 4: 567–568.

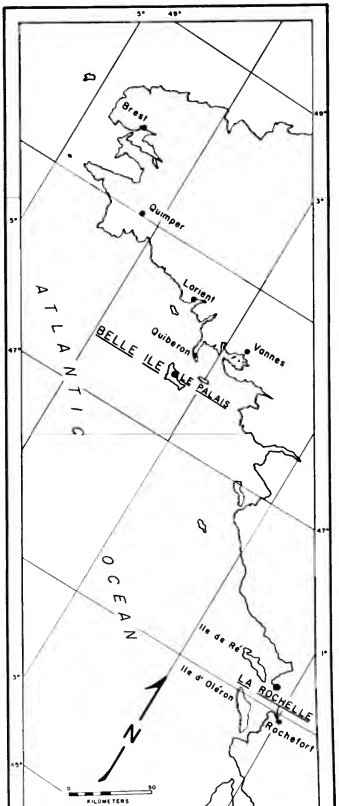
[33] Depping, *loc. cit.*; *Œuvres complètes* 6: 378–379, 501; La Roncière, *op. cit.* 5: 280–295, the latter a clear general account of the history of the expedition. The single clock employed was apparently one Delavoye had arranged to have made for Beaufort at the latter's request sometime after the expedition of 1668. Presumably the mechanically modified clocks Huygens was preparing in Paris were not ready in time.

and the integrity of his journal. Huygens did not see how the latter could have been falsified although he did raise the question.[34] Late in October, with his doubts on this score apparently satisfied, he reported that the news of his clock since its return from Candia continued to be very good. Delavoye was completely rehabilitated and roundly praised. To Huygens, the long oceanic voyage he had previously urged Colbert to authorize now seemed even more necessary than before.[35]

V

For more than two years the astronomers of the Academy of Sciences had been busily formulating plans for various overseas scientific expeditions, that were originally intended for Madagascar being the earliest in point of time.[36] Quite possibly the recommendation of suitable occasions for the testing of Huygens' marine clocks had, during this period, been requested of them. The fact that the official report of the Acadian voyage of 1670 was made directly to the Academy strongly suggests such a relationship.[37] Conceivably it was one which existed at the time of the trials made with Beaufort in 1668 and 1669. Unfortunately, the scanty records which have survived permit no unequivocal answer. This is equally true of the more important problem of the origin and history of plans which called for sending Huygens' clocks on *two* long voyages, the one presumably westward to Cayenne, the other south and east to Madagascar and the Indian Ocean.[38]

Plans for testing the clocks on an eastern voyage were rapidly perfected at the end of 1669. Royal policy, it so happened, provided a unique opportunity. A large naval squadron under Jacob Blanquet de la Haye, "Lieutenant General of the King in Madagascar and the Indies," was on the point of sailing on a three-year cruise in the region of Ceylon, India, and the Dutch East



MAP 1. The Atlantic coast of France. To illustrate the movements of the *Saint-Sébastien*, May 1–14, 1670.

[34] *Cf.* Huygens' memoir, Sur l'Essay des Horloges sur Mer par Monsieur la Voye dans le Vaisseau de Monsieur de Beaufort au voiage de Candie en 1669, *Œuvres complètes* **6**: 501–503; also **6**: 500; **18**: 633–635. The account of this voyage published in the *Horologium oscillatorium* (1673) has been reprinted, with French translation, in *Œuvres complètes* **18**. For the text and translation of the relevant passage, see pp. 116–119.

[35] *Ibid.* **6**: 379, 486, 514–515.

[36] For these and other developments from which the expeditions of 1671–1672 to Uraniborg and 1672–1673 to Cayenne ultimately emerged, see Olmsted, *Isis* **34**: 117–121, 1942.

[37] For the details of the report, see below, sec. XI.

[38] See below, p. 621 and n. 47.

Indies as one phase of French strategic dispositions in anticipation of future hostilities with the Dutch. Since May, 1669, the squadron had been fitting-out at Rochefort (map 1). Subsequently, on 3 December, De la Haye was officially named to the command of this "escadre de Perse." The same day, at Colbert's order, Delavoye, described in the official entry as a "scientist ordered to the East Indies for the trial of the pendulum clock for the [determination of] longitudes," was paid a stipend of 600 livres for the next four months, plus an additional 300 livres to cover the costs of transporting himself and his instruments to La Rochelle. Huygens' wish for the rigorous and sustained testing of his clocks on a long voyage at last seemed on the point of fulfillment.[39] (It is surprising, in view of previous mechanical difficulties with the clocks on shipboard, that a competent horologer familiar with the construction, maintenance, and repair of such instruments was not assigned to this voyage or to any of the later voyages on which the clocks were to be tested.)

Delavoye, however, seemed unable to stand his new prospects and temporary prosperity. His funds he complained were quickly "eaten up"; shortly he was dunning Huygens for more. The previous intimations of personal instability and untrustworthiness became unmistakable. As a result this *fripon*, as Huygens described him, was relieved of his assignment.[40]

With the departure of De la Haye's squadron imminent, a replacement had to be found at once. Although the evidence is inconclusive, the collaboration of the Academy of Sciences in meeting this situation was apparently requested. In any case, on March 10, 1670, Colbert could inform his cousin, Colbert de Terron, the royal intendant at Rochefort, that "the Academy of Sciences" had chosen one of its assistants to go to the East Indies "to make various astronomical observations in connection with others which are to be made here [in Paris], and to test the clocks which have been constructed for the determination of longitude at sea. . . ."[41] It was the King's wish Colbert added, that the assistant, Jean Richer, and his aide, Meurisse, make the voyage on board a ship of the royal squadron which was soon to sail for the east. Thus the Academy's earlier plan for a scientific expedition to Madagascar was not only suddenly revived but seemed on the verge of being carried out. The replacement of Delavoye as custodian of Huygens' clocks by a rising young astronomer, Richer, was apparently the decisive factor in this development.[42] At the last moment, however, something went awry: instead of Madagascar and the Indian Ocean, we shall find Richer and Meurisse journeying in obscurity westward to New England and Acadia. Small wonder that an occasional scholar should express annoyance with a voyage to the East Indies which somewhat incomprehensibly winds up in Acadia.[43]

VI

Richer and Meurisse were apparently able to prepare for the journey quickly. According to Colbert, their luggage and instruments had left Paris about a week prior to his message of March 10 to Colbert de Terron. Not later than 25 March the two observers were themselves at La Rochelle. On the day of the vernal equinox Richer found time to measure the height of the tides in the harbor. Just eight days later, the "Persian squadron" sailed from La Rochelle on its long and dangerous mission.[44] Despite all the

[39] Sottas, *op. cit.*, 43; Clément, *Lettres . . . de Colbert* **3**: 442 and n. 1, 461–470; **5**: 478; Guiffrey, *Comptes des bâtiments . . .* **1**: col. 379.

[40] *Œuvres complètes* **7**: 26–27.

[41] Clément, *Lettres . . . de Colbert* **5**: 294. The phrase "horloges et pendules" in the text is probably an error in writing "horloges à pendule." Charles Colbert, seigneur de Terron (1618–1684), a man of influence, was from 1666 to 1674 the *intendent général* in charge of all naval affairs relating to the Atlantic coast of France and to French fleets in Atlantic waters.

[42] Guiffrey, *Comptes des bâtiments . . .* **1**: col. 47[illegible] contains the interesting entry: "5 April 1670. To Sevin, maker of mathematical instruments, for various instruments which by our order he has furnished M. Richer whom we are sending to the [East] Indies to make some astronomical observations, 70# [livres] 10s." Meurisse (or Meurice) has been identified only as Richer's assistant from 1669 to 1673. He died at Cayenne in the latter year. On 20 February, 1670, Richer had been paid 336 livres as compensation for experiments and astronomical observations made on behalf of the Academy of Sciences during the latter part of 1669–167[illegible] *Ibid.* **1**: col. 270.

[43] E.g., by C. Wolf, *Histoire de l'Observatoire de Paris . . .*, 142–143, after quoting various seemingly contradictory statements found in contemporary documents.

[44] Clément, *Lettres . . . de Colbert* **5**: 294–295; Jean Richer, *Observations astronomiques et physiques faites en l'isle de Caïenne*, 89 (separately paged), in *Mem. Acad. Sci., 1666–1699* **7** (1), edition of 1729–1733; Sottas, *op. cit.*, 43. Subsequent citations of Richer's memoir will appear as *Observ. de Caïenne*, as found in the edition and volume of the Academy's *Mémoires* given above. De la Haye's squadron reached Madagascar 23 November, 1670.

reparations which had been made, Richer and leurisse were not aboard. The scientific ex- edition to Madagascar was off again. What ad happened to prevent it?

One ostensible explanation is provided by a eport which Colbert de Terron sent to Colbert n 3 April, five days after the departure of De la Haye's squadron. After discussing a variety of natters, the intendant laconically informed his hief of Richer's situation in these terms:

M. Richer's clock [clocks?] arrived here after the eparture of the Persian squadron. I think it will be ecided to send him to Acadia. It is a voyage from ast to west during which he will be able to make is experiments; and, if he returns in time, it will e possible for him to embark on *Le Breton* [for the ast Indies] in October.[45]

Yet it is possible this statement conceals more han it discloses. We know nothing of the clock r clocks which were late in arriving. We won- er whether Richer's equipment, which had ar- ived in ample time, did not include others which ould have been made to serve on the outward oyage, the clock(s) in question being forwarded y a later ship, such as *Le Breton*. Further, what elayed the arrival at La Rochelle of this par- icular clock, and what made it so vital to Richer's ission? Finally, why should Richer's destina- ion be a matter at this late stage for De Terron o decide? What lies behind this statement to olbert?

Certain elements in this situation can be dis- erned, though not as clearly as one would like. Iost important are the indications that various ersons of influence, De Terron among them, had easons to prefer that Richer be sent on a shorter, /esterly voyage, rather than on the voyage to the ndian Ocean in replacement of Delavoye. A oyage from east to west would, for one thing, nmediately provide substantial, cumulative dif- erences in longitude. In testing Huygens' ma- ine clocks, this would be an obvious advantage s against a voyage whose orientation would be primarily from north to south.[46] Furthermore, the results obtained would be known nearly a year sooner than those from a voyage to Mada- gascar which would require nearly eight months for the outward passage alone.

These or comparable reasons for sending Richer on a voyage west were bound to appeal strongly to the astronomers of the Academy. For some time past, Cayenne, rather than Madagascar, had been emerging as the preferred destination for the major overseas astronomical expedition deemed necessary to the furtherance of their observa- tional program. As early as May, 1669, Huygens was writing of observers about to be sent to "America." But by September of that year, Cayenne is the specific destination he mentions to his correspondent in England. Furthermore, by January of 1670 it has become a question of "our voyagers for the trial of longitude in east *and* west . . ." (my italics). Richer and Meu- risse, moreover, are officially described at this time in Colbert's account books as "mathema- ticians designated to go to Cayenne to make astronomical observations of utility to naviga- tion."[47]

In order to make the specific observations en- visaged by G. D. Cassini and others concerned in planning this "Brazilian expedition," Richer and Meurisse would need to sail from France during the autumn of 1671 or early in the winter of 1672.[48] If they accompanied De la Haye's squad- ron to Madagascar and the Indian Ocean what assurance was there that they would be back in time? Given the recent state of communications with this remote region, the prospects must have seemed slim at best.

Clearly, the astronomers of the Academy had grounds for intervening in the matter of Richer's destination. That they did so, either directly or indirectly, there is no evidence. The prestige of Cassini was already sufficient to enable him, had he so decided, to make representations person- ally to Colbert. And, indirectly, there was the matter of the readying in Paris of one or possibly

[45] Colbert de Terron to Colbert, 3 April, 1670, Biblio- hèque Nationale (hereafter given as B. N.), Mélanges e Colbert **176**: fol. 57 *verso*. The photostats of this orrespondence were obtained from the collections of the ibrary of Congress. Their loan is gratefully acknowl- dged. Whether De Terron meant one clock, or more han one (he writes "la pendule de M. Richer") is not ertain in the light of statements he makes elsewhere. ee below, n. 50.

[46] This aspect of the question was first called to my attention by a geographer and colleague, Professor Homer Aschmann. I am indebted to him for a number of helpful suggestions about some of the later portions of the paper.

[47] *Œuvres complètes* **6**: 427–428, 440, 486; **7**: 4; *Comptes des bâtiments . . .* **1**: col. 476.

[48] Greatest urgency attached to the proposed observa- tions of Mars, which in the fall of 1672 would, after an interval of about fifteen years, be at the point in its orbit nearest the earth. *Cf.*, Olmsted, *Isis* **34**: 121–122, 1942.

two of Huygens' marine clocks for Richer to use. It is tempting to infer that members of the Academy had a hand in making certain that "unavoidable delays" kept "M. Richer's clock" from reaching La Rochelle until after the departure of De la Haye's squadron. Had Huygens, for his part, wished to prevent any such machination—assuming there was one—he could have done nothing personally to counter it. Late in January or in February, he had become seriously ill and for many months thereafter could accomplish virtually nothing. Indeed, sometime during September or October, he returned to Holland to convalesce for an additional nine months.[49]

De Terron, being on the scene at La Rochelle, was obviously in the most favorable position to encourage or to effect a last-minute change in the plan to send Richer to Madagascar. He appears, moreover, to have had a specific reason for favoring such action. This is at least implied in a letter of 14 April written to Colbert:

> I have already had the honor to write you that Richer will embark with his clock [clocks]? on the *Saint Sébastien* for Acadia. Deshayes will also sail on the same vessel with the instrument that he has made in Paris. It is to be hoped that from the contact of these two men, who are embarking on good terms, knowledge will result with which you may be satisfied.[50]

To what extent, one wonders, was the situation revealed in this communication purely fortuitous? Colbert, to be sure, must have had some idea of what was impending and evidently interposed no objection; possibly he had some part in the arrangement. Yet, in view of De Terron's previous relations with Deshayes, it seems probable that any initiative would be likely to come from the intendant rather than the minister.

Deshayes is a distressingly obscure figure.[51] He has been described as an impoverished professor of *mathématiques* at Rouen who grew tired of trying to conduct courses to which no one came, and instead attempted to find a market for a "discovery" which he claimed to have made concerning the determination of longitude. To this end he apparently informed De Terron quite fully of his proposed method sometime during 1669. De Terron, who was himself interested in the improvement of navigation, and well aware of his cousin's preoccupation with the subject, must have been favorably impressed.[52] At least he sent his new protégé to Paris to wait on the minister. Colbert in turn passed Deshayes on to the Academy of Sciences for interrogation.

At the first autumn meeting of the Academy for the discussion of questions of *mathématiques*, on October 16, 1669, Charles Perrault, *Controleur des Bâtiments*, informed the members of the matter. Deshayes, he said, had represented to M. Colbert that he had found an exact method for the determination of longitude at sea. The minister had therefore asked Deshayes to present his method to the Academy. Recalling their experience of the preceding February with a certain Jacques Graindorge, the prior of Culey in lower Normandy, who was likewise a legacy from Colbert, the members must have been prepared for the worst. What they heard from Deshayes was probably better than they had anticipated.[53]

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[49] *Œuvres complètes* 7: 9–10; Brugmans, *op. cit.*, 67. No letter written by Huygens between 22 January and 15 October, 1670, is known.

[50] Colbert de Terron to Colbert, 14 April, 1670, B. N., Mélanges de Colbert **176**: fol. 91 *verso*. Again, as in the letter of 3 April quoted above, Colbert de Terron writes "la pendule," a further use of the singular form which adds to our uncertainty since, as we shall see, Richer unquestionably carried *two* clocks with him on the Acadian voyage.

[51] Careful inquiry has failed to elicit any biographical details concerning Deshayes. Until specific information can be found, the temptation to identify him with or relate him to the Jean Deshayes who was a member of a French scientific expedition of 1681–1682 to Gorée and the West Indies, and who between 1685 and his death at Quebec in 1706 was twice in Canada, where he did important cartographical work, must be resisted. However, dates alone do not rule out either the possibility that Deshayes and the better known Jean Deshayes were one and the same man, or that they may have been closely related, e.g., father and son.

[52] Didier-Neuville, Les établissements scientifiques de l'ancienne marine . . ., *Revue maritime et coloniale* 62: 459–460, 1879. In a letter of 21 April, 1670, to Colbert, B. N., Mélanges de Colbert **176**: fol. 105 *verso*, De Terron indicates a hope to see a school of "hydrographie et pilotage" established at La Rochelle, in part to maintain the morale of young naval officers by keeping them busy while in port. *Mathématiques* at this period embraced astronomy, navigation, mechanics, optics, etc., as well as geometry and the other branches of mathematics narrowly defined.

[53] Acad. Sci., Registres **5**: 184 ff., 1669, under date of 16 October. The rumor which had reached Oldenburg in England as early as 6 May, 1669, that the Academy on behalf of the king, had already offered Deshayes 160,000 livres for his discovery, *Œuvres complètes* 6: 427, is reminiscent of the exaggerations which gathered around Neystett in this same period. It reinforces previous indications of the seriousness of official and popular interest in the problem of longitude and of the large rewards currently talked about. Graindorge was rec-

The method proposed was in no sense original. consisted of finding the longitude of a given nt by means of the observation of the distance m the moon to the sun by day, and from the on to a star by night; in short, by the method "lunar distances." However, as the members the Academy rightly insisted, the state of ronomical knowledge in 1669 did not permit construction of sufficiently accurate tables of movements of the moon. In consequence, longitudes determined by this method would seriously in error. Huygens' opinion, more-r, was that Deshayes knew less about the hod he was putting forward than others who previously proposed its use.[54]

To the objections formulated against his pro-al, Deshayes returned written answers which re discussed by the Academy at the meeting 23 October. Thereafter an unfavorable report the practicability of Deshayes' proposition was mitted to Colbert.[55]

The matter evidently did not rest there. For thing, while still in Paris, Deshayes had ered "two large instruments, each of a diame-of two feet, and made by a certain Rousselot lives in the faubourg Saint-Germain"—at ose suggestion and at whose expense it is ossible to say.[56] In the second place, De ron could hardly have announced to Colbert April, 1670, that Deshayes was soon to embark Acadia unless tacit consent to a trial of his method had previously been given. The idea of having Richer and his assistant sail on the same vessel seems most likely to have been De Terron's. Could this have been the decisive element in the "delay" in the arrival of Richer's clock at La Rochelle the previous month?

An additional factor in determining the intendant's action may have been the attitude of the commander of "the Persian squadron," who was openly contemptuous of Richer's mission and reluctant to accord this accredited representative of the Academy the facilities and personal recognition stipulated by Colbert in his letter of 10 March. If the reports of the naval engineer, Massiac de Sainte-Colombe, based on conversations with Richer at La Rochelle are to be trusted, De la Haye's attitude and actions boded ill for the trial of Huygens' clocks. How far this situation influenced De Terron in substituting the Acadian voyage, we should like to know. Possibly one of the functions of this brief journey to North America was, in De Terron's eyes, to keep Richer suitably employed until he could be sent to Madagascar with a more sympathetic and cooperative commander. For, as he indicated to Colbert on 3 April, if Richer got back from Acadia in time, he could proceed to the east on board *Le Breton* in October. A "delay" in the arrival of Richer's clock(s) until after the departure of the East Indian squadron may, for the added reason of De la Haye's attitude, have appealed to De Terron as serving the legitimate interests of all parties.[57]

VII

However informative the apparent reasons for Richer and Meurisse not being aboard when De la Haye sailed in March, 1670, the change in their destination is not what gives the episode its

he "longitudinaires" of 1668–1669 referred to above, '7. The *procès-verbaux* of the Academy deal at th with the episode in which he was involved: istres 3: 261–272 (20 February, 1669), 273–278 (27 ruary, 1669), 279–282 (6 March, 1669), 1668–1669. atin memoir of 39 pages submitted to the Academy ained not only a method for longitude which, ac-ing to Huygens, was based on the assumption of thing sought, but also a "perfect meteorology," the cause of the wind and tides, whether the earth or sun turns, etc.—views which Huygens alleged were lrawn from Graindorge's "astrology."

Œuvres complètes 7: 4.

Acad. Sci., Registres 5: 184–190 (16 October), 197 (23 October), 1669; and *cf. Œuvres complètes* 33, 534.

Quoted by Didier-Neuville, *loc. cit.*, n. 52, above, a letter of 14 April, 1670, by a naval engineer, siac de Sainte-Colombe, who saw Deshayes—as well ;icher—at La Rochelle, and judged him a very sensi-person, though doubting the probable success of his iod for determining longitude. The nature of iayes' "instruments" is entirely unknown. That they esented a crude form of sextant or octant of two-foot is is not impossible. But why should this have made "large"?

[57] *Cf.* Didier-Neuville, *loc. cit.*, n. 52, above, who bases his conclusions on the extant letters of Sainte-Colombe; Clément, *Lettres . . . de Colbert* 5: 294; De Terron to Colbert, 3 April, 1670, *loc. cit.*, n. 45, above. Colbert's stipulation regarding Richer was that he should not only receive all the *commodités* which his mission required, but be given a place at the captain's table as well—a source, perhaps, of some of De la Haye's apparent resentment. The continued resolution to send Richer to Madagascar, despite the possible consequences for the projected 1671–1672 scientific expedition to Cayenne on which the Academy planned to send him, is a point of some interest. One would like to know the source of the apparent pressure. Did it result from an inclination on the part of Colbert and/or his cousin, to give top priority at this point to scientific enterprises which were centered narrowly on the solution of the problem of longitude at sea?

primary interest for the history of navigation. Rather it is the decision to send Deshayes *with* Richer on the same vessel. An early voyage during which it was intended to test the two principal methods by which the riddle of longitude at sea was ultimately resolved would, by its very singularity, command the historian's attention. This would still be the case even if chance had played a larger part than we now think in bringing Richer and Deshayes together at La Rochelle.

De Terron, once the decision revealed in his letter of 14 April to Colbert had been reached, was inclined to gloat at the prospect of having the two men test their respective methods on board the same vessel. For, as he subsequently wrote the minister, "one will provide the best possible check upon the other."[58]

Just what considerations had determined De Terron's choice of ship and destination for his two "longitudinaires" is not clear. As it happened, other posibilities than the *Saint-Sébastien* and Acadia were open to him. Three royal ships were at La Rochelle awaiting favorable weather before sailing for destinations in French North America. The *Gédéon* was for Quebec, carrying back to Canada the famous intendant, Talon. *Le Sigournois* was sailing for Plaisance with the governor-designate of Terre Neuve (Newfoundland) aboard. Lastly, the *Saint-Sébastien* was scheluled to go to New England and Acadia with some infantry, a military engineer, and—most important of all—Hector D'Andigny, Sieur de Grandfontaine, a naval captain who had recently been named plenipotentiary of Louis XIV for the belated surrender of Acadia by the English under the treaty of Breda of 1667, and civil and military commander in the province as well.[59] Possibly the date at which the last-named vessel was expected to return to La Rochelle was a factor in De Terron's decision.

Stormy and changeable weather held the *Saint-Sébastien* at La Rochelle all through April. On the first day of May, however, De Terron was able to inform Colbert that her captain, La Clochetrie, expected to sail at once. Ten days later word had been received that the ship had put in at Belle-Île (map 1) after a heavy storm "without further incident."[60] What mischance was thus alluded to we do not specifically know.

In all probability Richer observed the latitude of Belle-Île on this occasion.[61] Assuming the observation(s) to have been made at the island's principal town and port, Le Palais, whose latitude is approximately 47° 21′ 35″, Richer's figure of 47° 21′ is surprisingly good—well within the limits of accuracy of ± 1′ characteristic of the best work during the early 1670's—the period of the gradual introduction of "telescopic sights" and of noteworthy improvement in the knowledge of refraction and parallax.[62]

[58] Colbert de Terron to Colbert, 1 May, 1670, B. N., Mélanges de Colbert **176**: fol. 127 *verso*.

[59] The details are in the dispatches of Colbert de Terron to Colbert, B. N., Mélanges de Colbert **176**: *passim*, and, to a lesser extent, in Colbert's letters to the former in Clément, *Lettres . . . de Colbert* **3**[1]: *passim*. The King's order to Grandfontaine to command for three years in Acadia under the royal governor and lieutenant-governor in Canada, at the time the Sieur de Courcelles, was dated 20 February, 1670. Paris, Archives de la Marine, B[2], **10–12**: fol. 10–11. Grandfontaine remained in Acadia until 1675, when he was recalled.

[60] Colbert de Terron to Colbert, 1 May, 1670, 11 May, 1670, B. N., Mélanges de Colbert **176**: fol. 127 *verso*, 153 *verso; Œuvres complètes* **7**: 27. Chadeau de la Clochetrie (the orthography of the name varies considerably) was a "capitaine de frégate" from 1666 to 1671, becoming "capitaine de vaisseau" in the latter year. He died in 1696. The *Saint-Sébastien*, which he commanded in 1670, was listed as 250 tons burden and 16 cannon. In 1664, when she served as vice-admiral's ship in a fleet which went to the West Indies, the *Saint-Sébastien* carried a total of 153 crew members, soldiers and passengers. On a voyage to the same area in 1671 the total "équipage" was 120, viz., 20 "officers mariniers," 60 sailors, and 40 soldiers. S. L. Mims, *Colbert's West India policy*, 86, New Haven, Yale University, 1912, Yale Historical Studies **1**; Mémain, *op. cit.*, 920.

[61] This observation is known to me only by a letter of 16 December, 1679, from Jean Picard to J. D. Cassini, Observatoire de Paris, Manuscrits, B. 4. 12, in which there is a reference to "the elevation of the pole observed at Belle Isle [*sic*] by M. Richer of 47° 21′." Possibly Richer was at Belle-Île on some occasion after 1670, yet no reference to astronomical observations made by him subsequent to 1673 has been noted. In that year he returned from his two-year expedition to Cayenne, and, indicative of changing times, was shifted from the service of the Academy of Sciences to that of fortifications and military engineering under the celebrated Vauban. That he visited Belle-Île in this capacity, and made an astronomical observation or two on the side, is a possibility, but only that.

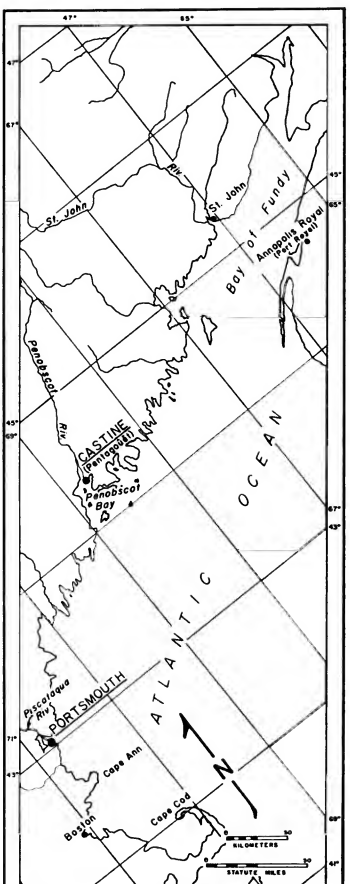
[62] Surprising accuracy was achieved at this time in observations made by astronomers of the Academy, an accuracy twentieth-century commentators sometimes find hard to accept. (This accuracy was likewise typical of the work of certain conservative astronomers like Hevelius, who continued to use large quadrants which did not have the advantage of "telescopic sights.") Apparently the nature of traditional instruments and the conditions of observation had long required and been productive of greater keenness of vision and observa-

n May 14 La Clochetrie wrote De Terron
1 Belle-Île that all was well and he was ready
ut to sea again.[63] Nevertheless, in one re-
t disaster had already struck. The heavy
n encountered by the *Saint-Sébastien* out of
Rochelle had resulted in serious damage to
clocks with which Richer had been entrusted.
ore the storm abated, both had stopped. In
tion, one clock seems to have suffered a fall
been more or less ruined. Neither was
n in operation at any time during the outward
he return passage.[64]

e Terron's great hopes for the voyage had
in part been dashed almost within sight of
French coast. His expectations in regard to
hayes' success must in the end have proven
lly illusory. But here the sources afford us
further aid, no trace of Deshayes or of the
rvations he may have made. Indeed there is
positive evidence that he was actually on
d when the *Saint-Sébastien* sailed; but neither
ere evidence that he was not. The curtain
imply rung down. The drama of the two
gitudinaires" and of the trial of their rival
nods ends in a conspiracy of silence almost
re it has begun.

VIII

nothing further can be learned regarding
attempts to determine longitude during the
edition to Acadia, the same is not true about
rvations of latitude. In addition, a number
etails about the voyage itself can be gleaned.
s we know that by July the *Saint-Sébastien*
at or near Boston. There, on the seventh
of the month, Grandfontaine signed a treaty
the British governor of Acadia, Sir Thomas
iple, providing for the full restitution of the
ny to the French crown.[65] To complete the



MAP 2. The New England and Acadian coast. To illustrate the movements of the *Saint-Sébastien* and the related events of July and August, 1670.

1 skill than is assumed or achieved now that me-
cal and optical advances in instruments have made
unnecessary. This general fact, plus Richer's prior
rience, the quality of his instruments, and the
lards of accuracy he later achieved at Cayenne, make
ong case for placing considerable reliance on the
minations of latitude he made in 1670. (See below,
VIII, IX.) It would be superficial to hold that
as merely lucky in certain random observations.
Colbert de Terron to Colbert, B. N., Mélanges de
ert **176**: fol. 155.
Loc. cit., n. 63, above; *Œuvres complètes* **7**: 54–55.
The text of the treaty is in *Collection de manu-*
. . . et autres documents historiques relatifs à la
elle France **1**: 198–199, 4 v., Quebec, 1883–1885.
he background of the surrender, *cf. ibid.* **1**: 187–195,
98; *Recueil des instructions données aux ambas-*

transfer, Grandfontaine and his party then sailed northward (map 2).

By July 16 the *Saint-Sébastien* had either reached or passed the general vicinity of the estuary of the Piscataqua River, site of modern-day Portsmouth. This we learn from Richer's report on his scientific expedition to Cayenne in 1672–1673, a voyage on which he was again the designated agent of the Academy of Sciences. In his account of this expedition, published in 1679, Richer recapitulates observations of the tides which he made at Cayenne, as well as in North America in 1670.

I shall add to these observations of the flux and reflux of the sea made at Cayenne, those which I made in the year 1670 on the coast of Acadia in Canada, and on the coast of New England. . . . On the coast of New England, in the harbor of a place called Piscataway [Pescatoué], on the open ocean, whose latitude is 43° 7′ N., I observed that the tide was high at 11:15 A.M., 16 July 1670, the day of the new moon.[66]

This statement warrants an attempt to fix more precisely the point on the New England coast near which the *Saint-Sébastien* was presumably at anchor on July 16 (map 3). Unfortunately, numerous difficulties stand in the way. First, none of the early maps which have been examined provides a clue to the location of a settlement which a contemporary Frenchman would know as Pescatoué, Pescadoué, or possibly Piscatewai; an Englishman presumably as Piscataway.[67] Moreover, there is some evidence that, during the seventeenth and even the eighteenth century, these early forms of Piscataqua were used quite indifferently to refer to a large, undefined area lying to the north of the estuary.

A more reliable guide in interpreting Richer's figure of 43° 7′ N. for the latitude of the harbor in question is provided by the qualifying phrase which tells us that the harbor of Pescatoué is "sur le bord de la grande mer." This statement, made by an experienced scientific observer, must be considered virtually decisive. Thus any of the inland harbors, between, say, 43° 3′ latitude on the south and 43° 11′ on the north, in which Richer's observations might conceivably have been made, can hardly be considered to lie "on the open ocean" and must be ruled out.[68] This includes the large number of points in the estuary of the Piscataqua itself lying between 43° 3′ and 43° 6′ N. latitude. The estuary of the York River, whose mouth is at roughly 43° 8′ N., seems similarly excluded, as does that of the Cape Neddick River at 43° 11′ N. In the latter case, it is doubtful whether the region designated by contemporaries as Piscataway can properly be considered to have embraced the territory lying to the north of the York River. This would be additional reason for rejecting both the York and Neddick estuaries as possibilities. Nor is it likely Richer's figure for the latitude of the site of his observations was in error by as much as the 3 to 4′ necessary to make either the Piscataqua or the Neddick serious possibilities.

Three points "on the open ocean" appear to remain (map 3). From south to north these are the cove at Seapoint, whose latitude is roughly 43° 5′ 25″ N.; what is now known as Brave Boat Harbor at 43° 6′ N.; and Godfrey's Cove at 43° 7′ 10″ N.[69] The difference in latitude between the first and third of these sites is 1′ 45″.

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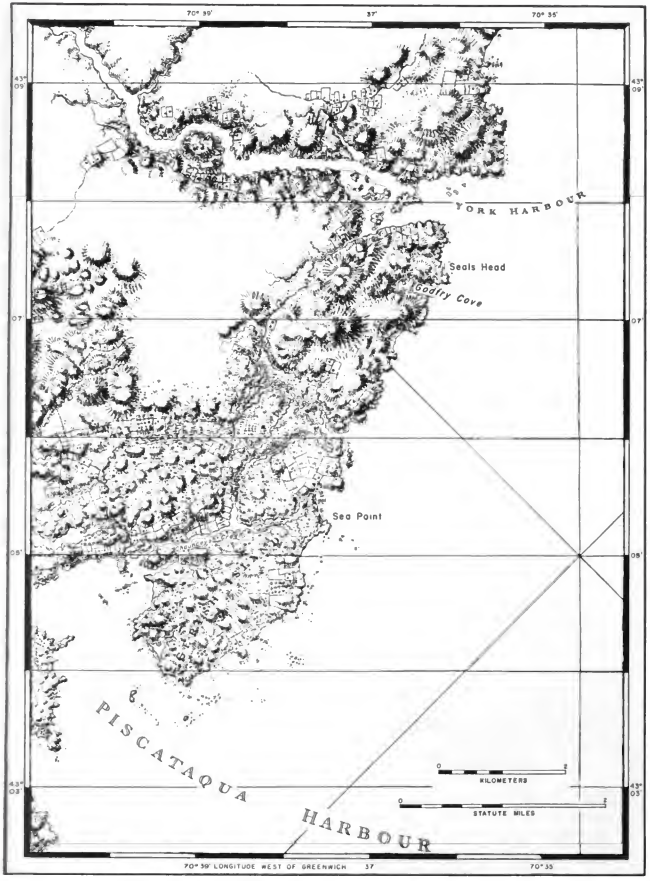
sadeurs et ministres de France depuis les traités de Westphalie . . . **25**: 19–20, 23–28, 86–88, 454–455, Angleterre, 2; Calendar of State Papers, *America and West Indies*, 1661–1668: nos. 1598–1600, 1635–1638, 1641, 1643, 1654, 1669–1701, 1709, 1808, 1815, 1868, 1877, 1898; 1669–1674: nos. 4, 23, 32, 69, 95, 384. On Sir Thomas Temple (1614–1674), consult *Dictionary of national biography* **19**: 520. The interrelationship between the *détente* over Acadia and the West Indian island of St. Christopher, the move for a commercial treaty, and the negotiations which in May and June, 1670, culminated in the celebrated secret treaty of Dover between Louis XIV and Charles II should not be overlooked. The best general history of the French in Acadia is E. Lauvrière, *La tragédie d'un peuple: histoire du peuple acadien* . . ., nouv. edn., 2 v. Paris, H. Goulet, 1924.

[66] Richer, *Observ. de Caïenne*, 89. The name "Pescadoué," using a "d" in place of Richer's "t," appears on a French manuscript map of 1650, but without any geographical detail. The map is reproduced in Justin Winsor, *Narrative and critical history of America* **3**: 382, 8 v., Boston, 1884–1889. Interest in the tides, as previously suggested, was widespread during the seventeenth century. Auzout's proposals to the Academy of Sciences in 1667 for a scientific expedition to Madagascar included careful observation of the tides and of ocean currents. See above, p. 616, and n. 23.

[67] The form, Piscatewai, appears on a map from the 1688 edition of Blome's *Amérique*, reproduced in Justin Winsor, *Cartier to Frontenac* . . . . 346, Boston and New York, 1894. The variant spelling, Pescadoue rather than Richer's Pescatoué, has been encountered in two or three documents of the post-1670 period as well as on the map of 1650 referred to in n. 66.

[68] For more detailed information about this portion of the Maine-New Hampshire coast, see the United States Geological Survey topographic map of the York Quadrangle, used in the edition of 1920.

[69] For additional detail on the places and latitudes indicated, see the map of the York Quadrangle referred to in n. 68, above.



MAP. 3. The New England coast from *circa* latitude 43° 2′ N. to 43° 10′ N., after a map of 1779 by J. F. W. Des Barres.

the greatest deviation from Richer's figure of 43° 7′ is in turn 1′ 35″ in the case of Seapoint. Both differences are in excess of the error of less than 1′ attained by Richer's values for the latitude of Belle-Île and that of Pentagoüet in Acadia, the latter of which will be discussed hereafter. Moreover, in both instances Richer's value for the latitude makes it too small rather than too large. Thus, if a systematic error were involved, through use of the same instrument at each of the three places, then the latitude of Pescatoué—particularly if it is the mean of two or more observations—should also be too small rather than too large. Yet, as we have seen, any point north of Godfrey's Cove, at 43° 7′ 10″ N. latitude, can hardly be made to fit either Richer's description of the harbor or the name of the place at which his several observations were made. In this case we must apparently admit the possibility of an error of ± 2′—possibly more—in the latitude which Richer reports for Pescatoué. If he made only a single observation, as seems probable, or used a different instrument, such a limit of error would be altogether normal. Thus, in terms of latitude alone, we can only conclude that any one of the three points named might have been the place known to Richer as Pescatoué.[70] On other grounds than latitude, either Seapoint or Godfrey's Cove appears the more likely location.

Seapoint, particularly as shown on a 1779 map by J. F. W. Des Barres in the *Atlantic Neptune* (map 3), has several claims to be considered the site in question.[71] First, there is the question of proximity to the estuary of the Pescatoué, a good reason for the application of the name to a nearby settlement. Further, in the seventeenth century the cove was pretty certainly larger than now. It would accordingly provide better protection for ships of the size of the *Saint-Sébastien*. A further argument in Seapoint's favor is its closeness—not much more than half-a-mile—to an east-west arm of the Piscataqua estuary now known as Chauncey's Creek. If the captain of the *Saint-Sébastien* preferred the greater shelter of the estuary, a trip by small boat along the Creek, followed by a walk overland to Seapoint with the requisite instruments, might conceivably have been managed. The fact that there is today some settlement proximate to the "harbor" at Seapoint is a tenuous added argument in its favor.

The case for Brave Boat Harbor is weakened by its topography. The harbor consists of a fairly narrow entrance behind which lies a small estuary in which a ship might drop anchor. Possibly, merely by contrast to the magnitude of the estuary of the Piscataqua, with its numerous inland havens, the Harbor might on a first visit to this coast have been considered to lie "on the open ocean." Yet the settlement of Pescatoué, if situated here, would pretty certainly have lain somewhere on the inner reaches of the estuary. How then could the phrase, "on the open ocean," properly have been applied to it? In this respect Brave Boat Harbor appears to fit Richer's basic condition somewhat less well than either Seapoint or Godfrey's Cove.

The latter offers us the third and presumably final possibility. As a harbor, the cove was doubtless larger and capable of providing better shelter in 1670 than it could today after almost three centuries of wave action on Seal Head Point, plus the attendant silting of the Cove itself. On the other hand, Godfrey's Cove is still farther removed than Brave Boat Harbor from the Piscataqua River and from the contiguous region which might most readily lend its name to the settlement. Possibly this consideration tilts the balance, however, slightly, in favor of Seapoint as the most likely site for Richer's observations on July 16, 1670—observations made "on the coast of New England, in the harbor of a place called Pescatoué, on the open ocean. . . ."

IX

Before the end of July, Richer's ship had reached Penobscot Bay (map 2). There the French had a small fort called in this period Pentagoüet, "on the river of the same name," as Richer later wrote.[72] The fort occupied a site

[70] The time of high tide observed by Richer can apparently offer no help in differentiating the three places. The Director of the United States Coast and Geodetic Survey has kindly informed me that the time of tide would be almost simultaneous all along the coast from the Piscataqua to Cape Neddick. In support of Richer's observational accuracy, he further indicates (letter of 11 August, 1959) that the "time of 11:15 A.M. for high water on 16 July, 1670, appears to be reasonable. . . ."

[71] Des Barres' map, as map 3 indicates, names as well as locates "Sea Point" and "Godfry Cove." Brave Boat Harbor is not named or perhaps as accurately represented on the map as the other two appear to be. My photostat is from a Library of Congress copy of *The Atlantic Neptune*, copy 2, vol. 3, map no. 12. Presumably this is from the collection published in London in 1781 as pt. 2 of vol. 2 of the *Neptune*: Joseph F. W. Des Barres, *Charts of the coast and harbours of New England from surveys taken by Samuel Holland. . . .*

[72] *Observations de Caïenne*, 89, as cited in n. 69, above

MAP 4. The fort and harbor of Pentagoûêt in Acadia, after a French map of *circa* 1688. From a copy in the Map Division of the Library of Congress.

practically at water's edge, on low cliffs dominating the harbor (map 4). During the eighteenth century, both harbor and town came to be known as Castine, following nearly fifty years in which their history was intimately associated with the colorful career of Jean Vincent d'Abbadie, Baron de Saint-Castin (1650–1712). In the later eighteenth century the harbor lay under the guns of Fort George, which the British built in 1778 on the high ground in the center of Castine's jutting headland. The United States Geological Survey map of the Castine Quadrangle gives Fort George, whose ruins are still a prominent landmark, a latitude of approximately 44° 23′ 25″ N. The latitude of the site of Fort Pentagoüet, known to have been situated somewhat west of the present waterfront area of the town roughly half-a-mile from Fort George, cannot have been far from 44° 23′ N.[73]

On 31 July and 4 August, respectively, Richer observed the height of the tides at Pentagoüet. During his stay he also made observations of the latitude, reporting it to be 44° 22′ 20″ N.[74]

This figure is in error by something over 40″, thus conforming well with the apparent error in the latitude of the harbor of Belle-Île presumptively observed by Richer on the outward voyage from La Rochelle.[75] Perhaps, as seems likely to be the case with the latitude of Belle-Île, Richer's value for that of Pentagoüet is the mean of several observations. For at both these places, unlike Pescatoué, where the visit was presumably brief, Richer had the time in which to get several readings. One important reason for doing so would have been his involvement in the trials currently being made of telescopically equipped instruments devised a few years earlier by the Abbé Jean Picard, instruments which were bringing revolutionary new refinement and accuracy to French observational astronomy.

It is accordingly significant to note that Richer reports the latitude of Pentagoüet in what, even in 1679, was not yet common usage for observations of celestial altitudes. Specifically, he gives his observation(s) in degrees, minutes, and seconds of arc, rather than in degrees and minutes only. At the time the latitude was observed, this was a radically new departure, practicable in serious astronomical observation only since Picard began using a special 28-inch quadrant which was probably the first instrument for measuring large angles equipped with a complete telescope and cross-hairs.[76] We wonder, of course, whether, either in preparing his memoir for publication in 1679, or at some earlier date, Richer modified his observed value for the latitude of Pentagoüet, applying to it a correction growing out of improved knowledge of the instrument he had employed. Further, was the instrument he used one of the new telescopic type, and hence capable of observations of such refinement? Could it, in fact, have been Picard's original 28-inch quadrant which we know Richer used extensively at Cayenne in 1672–1673—an instrument he more than likely had with him at La Rochelle in 1670 in anticipation of his scheduled three-year expedition to the East Indies? These are interesting questions. The sources do not provide the evidence with which to answer them.[77]

[73] Additional geographical detail will be found on the U. S. G. S. map of the Castine Quadrangle mentioned in the text, and used in the edition of 1904. The most extensive information on the history of the settlements and forts at Castine is in G. A. Wheeler, *History of Castine, Penobscot, and Brooksville, Maine; including the ancient settlement of Pentagoët*, Bangor, 1875. Part III of the work, "Documentary," reprints a valuable selection of documents, of which roughly forty relate to the pre-Revolutionary period. Wheeler's later, more detailed studies of the old Fort of Pentagoüet are in *Collections and Proceedings of the Maine Historical Society*, second series, **4**: 113–123, 1893. For detailed discussion of the fort and its remains, see pp. 118–121.

[74] *Observations de Caienne*, 89, as cited in n. 66, above. The tidal observation on 31 July was made the day of full moon, the tide being high at "9 or 10 seconds before noon." So precise a figure suggests that solar observations in connection with the determination of both local time and latitude (from the meridian altitude of the sun) were in progress. The difference between low and high tide that day was 10 feet (pieds), although the tide on 4 August is said to have been "even higher than the other days."

[75] See above, p. 624.

[76] A summary account of Picard's introduction of what he and contemporaries knew as "telescopic sights" will be found in J. W. Olmsted, The "application" of telescopes: 1667 or 1668? *Sky and Telescope* **8**: 7 ff., 1948. A fuller discussion is in *Isis* **40**: 214–224, 1949. Earlier practice was to report altitudes, etc., to the nearest minute, or occasionally the half-minute.

[77] During 1669 and after, Picard was successfully employing telescopically-equipped quadrants and similar instruments for measuring angles in his celebrated and impressively accurate "measurement of the earth." He had been seizing every opportunity to test these new instruments. He thus had quite as much reason to wish to have a telescopically-equipped quadrant used on an expedition to Madagascar in 1670 in which Richer was involved as he did a year later when preparations were being made for the latter's expedition to Cayenne. It may be more than coincidence that Picard seems to have made no observations with his first, 28-inch telescopic quadrant *after* 3 March, 1670, and probably not for some little time before. It was during this first week in

The one fact which stands out in this discussion is that Richer's value for the latitude of Pentagoüet is correct within limits of accuracy of ± 1′, instrumental errors and those due to refraction included. And such limits, it may again be said, constitute the touchstone by which to judge the quality of contemporary astronomical work. Even though Richer did not have a special emplacement for his instruments, as he did at Cayenne, it seems unlikely that so good a result came merely from chance. Richer's skill as an observer, the presumable quality of his instruments, his involvement in the current efforts to improve standards of observational accuracy—all these, as previously indicated, have a bearing on the quality of the results of which he was capable when conditions were favorable. They appear to have been so at Pentagoüet. Thus Richer's figure of 44° 22′ 20″ N. for the latitude of the fort should probably be accepted as the most refined and accurate astronomical observation thus far made in North America, or—for that matter—in the western hemisphere; further, it may be considered the most accurate determination of latitude yet made in the New World. It seems doubtful that the latitude of any other North American point was known with comparable accuracy for at least another generation and probably longer.[78]

March, according to Colbert, *Lettres* . . . **5**: 294–295, that Richer's instruments and luggage left Paris for La Rochelle, ostensibly for the long voyage and expedition to Madagascar. P. Lemonnier, *Histoire céleste* . . ., 17, 20, and *passim*, Paris, 1741, is the source for Picard's observations at this period. The instruments Richer used at Cayenne are described by him in *Observations de Caïenne*, 5–6, 8.

[78] The principal contemporary French account of the North American coast from New England to the Gulf of Saint Lawrence offered nothing better than the statement that "the river of Pentagoüet is situated under the latitude of forty three and a half degrees. . . ." Nicolas Denys, *The description and natural history of the coasts of North America*, Paris, 1672, ed. and transl. W. F. Ganong, 247, Toronto, Champlain Society, 1908. An attempt in 1652 to establish the north boundary line of the Bay Colony, ordered by the General Court, had resulted in a determination of the latitude of the head of the Merrimac River which was in error almost 4′. S. E. Morison, *Harvard College in the seventeenth century* **1**: 211 and n. 3, 2 v., Cambridge, Mass., Harvard University, 1936. According to Samuel Williams in 1785, the earliest reported figure for the latitude of Cambridge was derived from observations of eclipses made in 1694 by Thomas Brattle. The resulting value of 42° 25′ Williams says was generally accepted until his own observations in 1784–1785, using a 2½-foot quadrant made by Sisson, showed the latitude of Harvard Hall to be 42° 23′ 29″. *Memoirs of the American Academy of Arts and Sciences* **1**: 62–69, Boston, 1785.

X

The relatively refined determination of the latitude of Pentagoüet is one specific accomplishment of Richer's first experience in scientific voyaging. The place at which it was made was situated in an area in which French interests, growing out of explorations initiated early in the century, were of long standing. Indeed, the history of French settlements at Pentagoüet dates back to 1613 in the days of Champlain, to whom, as his map of 1632 indicates, the river on which the fort was situated was known as the Pemetogoit; only in the next century, under English rule, did it difinitively become the Penobscot.[79]

After 1670, when Grandfontaine chose it for his headquarters, the strategic importance of the fort at Pentagoüet was increasingly recognized. In fact, from that time until just after the turn of the century, throughout the Saint-Castin period, either Pentagoüet or one of the forts on the Saint-John's River was regularly the post of France's principal garrison in Acadia and the seat of the provincial government as well. Only after 1700 was Pentagoüet surpassed in importance by the rising star of Port Royal.[80]

Thus the official surrender of the fort to the French on August 6, 1670, two days after Richer's second observation of the tides in the bay, was a political event of some consequence. For us its interest lies in the strong probability that Richer and Meurisse, the first persons associated

[79] For a contemporary account of the founding of the first French settlement, see *The works of Samuel de Champlain* **4**: 12–20, 6 v., Toronto, Champlain Society, 1922–1936; also *The Jesuit relations and allied documents*, ed. R. G. Thwaites, **8**: 130, 287, 73 v., Cleveland, 1896–1901. E. Lauvrière, *op. cit.* **1**, provides a good secondary account of the entire period. The map referred to is reproduced as plate 10 of the "Portfolio of Plates and Maps" which accompanies the edition of Champlain's works cited above. New Englanders were using the name Penobscot in the seventeenth century.

[80] J. B. Brebner, *New England's outpost: Acadia before the conquest of Canada*, 43, New York, Columbia University, 1927, Columbia University Studies in History, Economics, and Public Law, no. 293. Grandfontaine maintained his headquarters at Pentagoüet until he was recalled the year after a raid on the fort in 1674. On the role of the settlement and the fort during the Saint-Castin period, *cf.* L. J. Burpee, *The Oxford encyclopedia of Canadian history*, 566, New York, Oxford University, 1926; *Encyclopedia of Canada* **2**: 12, 6 v., Toronto, University Associates of Canada, 1935–1937. Additional information and bibliography is in Thwaites, ed., *Jesuit relations* **63**: 65, 299–300; **71**: 315–334, and *passim*; P. F. X. de Charlevoix, *History and general description of New France*, transl. J. G. Shea, **3**: 138–139, 186–188, 210–211, and *passim*, 6 v., New York, 1866–1872.

with the Académie Royale des Sciences to be charged with a mission overseas, and its first representatives to come to the New World, were present. That they were likewise at Port Royal on 2 September when it in turn was relinquished to the French seems less likely.[81] Had Richer visited that settlement, he would very probably have had an additional latitude and some further tidal observations to add to his scientific catch.

The probablity is strong that the *Saint-Sébastien* sailed for France directly from Pentagoüet sometime in August, well before the surrender of Port Royal. For on 18 September De Terron wrote Colbert that the vessel had entered the harbor of La Rochelle the previous evening, and that he was forwarding at once a letter and copies of memoranda sent by Grandfontaine.[82] Richer and Meurisse were presumably on board when the ship dropped anchor. At any event, Richer was able to round out his voyage across the Atlantic with some observations at La Rochelle on the occasion of the autumnal equinox, 21 September. On that day he observed the height of the tides in the harbor, just as he had exactly six months earlier when his departure for the East Indies appeared imminent. Sometime before the end of the year both he and Meurisse were again in Paris.[83]

XI

The official report to the Academy on the scientific results of the voyage can hardly have been easy or pleasant for Richer to make. To be sure, he had made some useful and commendably accurate determinations of latitude; he had obtained valuable information about the height of the tides on the eastern and western shores of the Atlantic; apparently he had done a little collecting; possibly he had accomplished other things besides.[84] But on the all-important subject of longitude, there was nothing but failure—disastrous failure, at least as regards the chronometric method—to report. The absence of Huygens, who was still convalescing in Holland, must have been a boon to Richer when he appeared before the Academy early in January, 1671, to present an account of his mission.[85]

The report at least had the merit of brevity. The storm which had been encountered soon after leaving La Rochelle, Richer indicated, had been too much for the clocks; thereafter nothing could be done with them. Of progress toward the solution of the problem of longitude by this method there was nothing to relate. Before success could be attained, he implied, further mechanical improvement of the clocks would be necessary.

This conclusion Huygens was not yet prepared to accept.[86] He had first learned of the failure

[81] Documents on the surrender of Pentagoüet and the other posts in Acadia are in *Collection de manuscrits . . . relatifs à la Nouvelle France* **1**: 199–202. The act of surrender of Pentagoüet and other documents relating to this post are reproduced by Wheeler, *op. cit.*, 254 ff., chiefly from the transcripts made in France by B. Perley Poore in the Archives of the Commonwealth of Massachusetts, esp. vols. **2**, **3**. The surrender of Gemesié or Jemseck on the St. John's River was received on 27 August by Grandfontaine's lieutenant, Soulanges, who likewise accepted that of Port Royal on 2 September—both from the same English representative, Capt. Richard Walker. E. Lauvrière, *op. cit.* **1**: 123; Charlevoix, *op. cit.* **3**: 138, n. 3.

[82] Colbert de Terron to Colbert, 18 September, 1670, B. N., Mélanges de Colbert **176**: fol. 319. Grandfontaine was evidently expected to inform his government promptly of the success or failure of his mission. To guard against the possible nondelivery of his reports and maps of Pentagoüet sent via the *Saint-Sébastien*, messengers carrying the relevant information were sent overland to the returning intendant of Canada, Talon, at Quebec, arriving about 10 November. *Cf. Collection de manuscrits . . . relatifs à la Nouvelle France* **1**: 194, 200–201, 202; Courcelles to Colbert de Terron, Quebec, 19 September, 1670, B. N., Mélanges de Colbert **176**: fol. 416 *verso*, 417, reporting on messengers *previously* sent to Acadia to get news of M. de Grandfontaine. One of the two known sketch maps of the fort of Pentagoüet in 1670 or thereabouts appears to have accompanied the memoir of 10 November, 1670, from Talon to De Terron, announcing the arrival in Quebec of the messengers from Acadia and reporting on the affairs of the province. A somewhat different map, probably of 1670, may be that sent directly to France by the *Saint-Sébastien*. The two originals are catalogued in H. Harisse, *Notes . . . à l'histoire, à la bibliographie, et à la cartographie de la Nouvelle France . . . 1545–1700*, 193, Paris, 1872; also G. Marcel, *Cartographie de la Nouvelle France . . .*, 26, Paris, 1885.

[83] *Observations de Caïenne*, 89; *Comptes des bâtiments . . .* **1**: col. 476. Meurisse was paid for the expenses of his trip from La Rochelle to Paris on 27 December, 1670, presumably some little time after reaching the latter city.

[84] In Acad. Sci., Registres 7: 124, 1675–1679, it is recorded that a member of the group, the botanist Marchand, showed the assembled company a plant called *Solanum acadiense*, which had been collected by Jean Richer. At Cayenne, Richer also did a little zoological collecting.

[85] The text of the report, like some of the correspondence relating to it, is apparently lost—probably with the Academy's *procès-verbaux* for the years 1670–1674. Its nature can be pretty well established from Huygens' letter of 4 February, 1671, *Œuvres complètes* **7**: 54–55.

[86] Later the validity of Richer's contention was indirectly acknowledged by Huygens when he attempted to change and improve the suspension of the pendulum

of the clocks by letter directly from Richer. Later a summary of Richer's report to the Academy—in all probability from the pen of the secretary, Duhamel—sent him into a rage. The fault, he vehemently insisted, was with the observers, not with the clocks, just as it had been on Delavoye's first voyage.

In a letter written on 4 February, Huygens was more specific. Richer's handling of the clocks, he declared, had been bad throughout the voyage. For want of a little oil, properly applied, the clocks had been needlessly damaged and afterward more or less ruined; for want of attention to the written instructions provided, they had not been started again after the storm so that they might be observed during the balance of the voyage. In short, he concluded, "the want of success on this occasion, as far as I can judge, stems more from the carelessness of the observers than from the failure of the clocks."[87] How Richer's report could have satisfied the members of the Academy, he could not comprehend. Basically, what Huygens would not yet acknowledge, as about a decade later he did, was that the adaptation of pendulum clocks to marine conditions was impracticable.[88]

XII

At the end of 1670 prospects for the success of Huygens' marine version of his pendulum clocks were dim indeed, particularly in comparison with those designed for use on land. The latter, subsequently labeled "astronomical clocks," had now attained a high degree of accuracy and were assured a brilliant future. In fact, when used in combination with two recent "inventions" by members of the Académie des Sciences, namely filar micrometers for measuring small celestial angles (1665–1666), and the quadrants equipped with "telescopic" sights for the more accurate measurement of large angles, as first brought into use by Picard from 1667 to 1669, the clocks were soon to revolutionize both the methods and the standards of observational astronomy.[89] It is at this point that modern, precise astronomy of position begins.

Moreover, in direct aid of navigation, astronomical clocks contributed vitally during the years before and after 1700 to the general renovation of cartography which has already been briefly described.[90] The practical success of G. D. Cassini's method for the determination of terrestrial differences of longitude, like the verification of his tables of the motions of Jupiter's satellites, hinged on their use. Thus in 1671–1672, employing accurate pendulum clocks of Huygens' design, Picard, who was on a scientific expedition in Denmark, and Cassini, who remained in Paris, were able to determine the difference in longitude between the Observatoire de Paris and the site of Tycho Brahe's famous, but long since destroyed observatory on the island of Hven. Shortly thereafter, as part of the Academy's next scientific expedition, Richer at Cayenne and Cassini at Paris were able by similar observations to fix fairly accurately the difference in longitude between these two widely separated stations. For cartography, at least, these were epoch-making accomplishments. Without Huygens' clocks neither would have been possible.[91]

used in his marine clocks. *Horologium oscillatorium* (1673), in *Œuvres complètes* **18**: 120–122.

[87] *Œuvres complètes* **7**: 54–55.

[88] *Ibid.* **8**: 197. For this change in Huygens' attitude, see below, p. 634. Huygens' correspondence attests a certain ambivalence about the prospects for the success of the marine pendulum clocks from as early as 1663, e.g., *Œuvres complètes* **4**: 432. While he was wavering, opinion in London in the Royal Society gradually crystallized against the pendulum clocks, following the lead given by Robert Hooke, an outspoken critic of the prospects for such clocks and a proponent of the use of a spring balance in place of a pendulum. *Cf. ibid.* **6**: 495; **7**: 6, the latter a letter from Oldenburg to Huygens in February, 1670. Hooke had made his position explicit in 1665, possibly with the intent of embarrassing Huygens, whose rivalry in the search for a solution of the problem of longitude he did not welcome. Before the Royal Society, on 15 March, 1664/65, Hooke remarked that, "in his opinion, no certainty could be had from pendulum watches for the longitudes, because, 1. They never hung perpendicular, and consequently the cheeks were false. 2. All kinds of motions upward and downward, (though it should be granted that the watches hung in an exact perpendicular posture) would alter the vibrations of them. 3. Any lateral motion would produce yet a greater alteration. . . ." Quoted in R. T. Gunther, ed., *Early science in Oxford* **6**: 238–239, 14 v., Oxford, Oxford University Press, 1923–1945.

[89] On this "revolution," *cf. Isis* **34**: 123, 1942; **40**: 214–215, 1949.

[90] See above, pp. 613–615 and n. 8, 9, 11, 12. To the references there listed, add the admirable article of L. Gallois, based on critical use of the principal manuscript sources, L'Académie des Sciences et les origines de la carte de Cassini, *Annales de géographie* **18**: 193–204, 289–310, 1909, the first installment being the pertinent one.

[91] The results of the Picard-Cassini observations are discussed in Picard, *Voyage d'Uraniborg, ou observations astronomiques faites en Dannemarck*, printed in vol. **7** of *Mémoires de l'Académie des Sciences de 1666 à 1699*. No critical study of this expedition has appeared. For the Cayenne-Paris longitude, see *Isis* **34**: 125 and n. 79, 1942. The use of the clocks in establish-

No such future was in store for the marine pendulum clocks. By 1671 it was becoming apparent that they were unlikely to offer an immediate solution of the problem of longitude at sea. Huygens must have felt this in his bones even while he was berating Richer for his handling of the clocks during the voyage to Acadia. To be sure, he might for many years make sporadic efforts to improve the design of the clocks in the interest of greater seaworthiness and increased accuracy. Yet earlier doubts and misgivings appear to have remained. As if to give them the lie, the great *Horologium oscillatorium*, which appeared in 1673, contained an optimistic account of the trial of the clocks during the voyage of 1669 to Candia.[92] However, in 1675, the Royal Society's *Philosophical Transactions*, following the Parisian *Journal des Savants*, reported Huygens as currently advocating a spiral spring as the most promising motive force for accurate portable clocks.[93]

The proposal was not entirely new. Huygens, and—more particularly—Robert Hooke, had been interested in spring balances at least a decade earlier.[94] At that period, Huygens, as has already been mentioned, was acutely conscious of the inequalities in the daily rates of his marine clocks even on land, and doubted they would ever be able to determine longitude with sufficient precision. Now these early doubts returned. "Because the . . . [pendulum] clocks necessarily suffer from the motion of a ship," he wrote in 1679, "there is more likelihood of success through the use of a balance wheel with a spiral spring."[95] This was at base his final decision—the decision of the man whose invention and perfection of the pendulum clock was already a landmark in the history of chronometry.

Because of the prestige his achievements conferred, Huygens, rather than Hooke, who had a tendency to promise more than he produced, appears to have been in the stronger position to influence the great horologists of the eighteenth century and to mark out the most promising path for them to follow. Yet, implicit in the conclusion he had reached was the postponement for nearly seventy years of the solution of the problem of longitude at sea. Not until two generations had passed would mechanical skill and scientific knowledge reach the point at which a marine chronometer of sufficient accuracy could be constructed; only then would knowledge of the movements of the moon have become both precise and extensive enough for the method of lunar distances to be successfully utilized.[96]

The return of the *Saint-Sébastien* from North America in the fall of 1670 could accordingly offer French scientists and government officials little hope that a solution to the riddle of longitude at sea was at hand. Yet, the significance of the voyage to Acadia was not on this account merely negative. Neither were its results, limited though these may have been. Useful observations had been accomplished and valuable experience gained by men who were shortly to undertake an epoch-making expedition to equatorial America.[97] Such beginnings as the voyage to Acadia were admittedly humble and unspectacular. Yet, because these beginnings grew out of, and in a sense epitomized, a widespread scientific concern with problems of oceanic navigation, they were not inauspicious, whether for navigation, cartography or astronomy. For French scientific voyaging the implications were even clearer and more immediate.

ing differences in terrestrial longitude was an essential element in Ole Roemer's determination at Paris in 1676 of the finite velocity of light. Roemer was brought to this discovery largely as a result of efforts to account for differences in the calculated and the observed times of the eclipses of Jupiter's satellites. The best account is I. B. Cohen, *Roemer and the first determination of the velocity of light*, New York, Burndy Library, 1942, a study originally published in the wartime issue of *Isis* **31** (84): 1940, the distribution of which was impossible outside of Belgium and Germany until after 1945.

[92] *Œuvres complètes* **18**: 116–119, as cited in n. 34, above. References to the continuation of efforts to improve the marine pendulum clocks are scattered throughout the volumes of this work, e.g., **18**: 120–122, 539–545. Yet materials in the latter volume suggest that after 1675 Huygens' energies were directed more to the development of a different type of marine clock.

[93] No. 112; *272–273*, 25 March, 1675, from the *Journal des Savants* of 25 February, 1675.

[94] Contemporary information regarding Hooke's proposals concerning spring-balance watches and their use in the determination of longitude is most easily accessible in R. T. Gunther, ed., *Early science in Oxford*, e.g., **6**: 10–20, 235, 238–239; **7**: 429; **8**: 146–150. The proposals made by Hooke in the Cutlerian Lectures for 1664 are discussed by R. T. Gould, *Marine chronometer . . .*, 24–26. On Huygens' early interest and later proposals, a rather involved subject, see *Œuvres complètes*, esp. **5**: 427, 486, 501, 503–506; **18**: 501–507, 522–525.

[95] *Œuvres complètes* **8**: 197; and *cf.* **4**: 432.

[96] These developments are well presented by Marguet, *op. cit.*, 134–260. For the chronometer this may be supplemented by Gould. For an instance of contemporary discounting of Hooke's assertions of what he could do about producing accurate portable watches employing a spring balance, see Oldenburg's comments of 25 January 1666, to Huygens, *Œuvres complètes* **6**: 7–8.

[97] The conditions and accomplishments of the expedition of 1672–1673 to Cayenne by Richer and Meurisse are discussed in my article in *Isis* **34**: 117–128, 1942.

COPERNICUS WAS NOT A PRIEST *

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. . . andranno raziocinando a rovescio, e argomentando in dispetto della evidenza delle cose.

Leopardi †

I. THE BIRTH OF A MYTH

HROUGHOUT the scriptural seventy years of iolas Copernicus' life (1473–1543) nobody called the founder of modern astronomy a t, nor did anybody ever do so for more than nty years thereafter. Then this mistake was made by Galileo (1564–1642) when, in his *er to the Grand Duchess Christina*, he de- ied Copernicus as "not only a Catholic, but iest and a canon."[1] For his statement that ernicus was a priest, the renowned Italian itist offered no evidence whatever.

a previous formulation he had characterized ernicus as "not only a Catholic, but a member ie regular clergy and a canon."[2] For his i that Copernicus belonged to the regular y, he likewise adduced no proof at all.

short, Galileo put forward two unsupported ations. More significant than his failure to ess them is his tacit withdrawal of his first ation (that Copernicus joined the regular y)[3] by substituting for it his second allega- tion (that Copernicus was a priest). Yet his second allegation rests on no firmer basis than his first. Both are the merest allegations, without any foundation in historical fact. Nevertheless, Galileo inserted the mislabeling of Copernicus as a priest, together with four other serious misstatements about the astronomer, in his *Letter to the Grand Duchess*.[4]

Galileo wrote this letter, probably in 1615, as part of his valiant but unavailing campaign to save his Church from committing the grave blunder of condemning Copernicanism as a heresy. Under these circumstances the letter was not printed. Its highly controversial nature, however, aroused such intense interest that it was reproduced in numerous handwritten copies.[5] Some two decades after the letter was composed it was printed for the first time, the original Italian being accompanied by a Latin translation.[6] An English version soon made Galileo's thesis that "Copernicus . . . was not only a Catholick, but a Priest"[7] available to the seventeenth century in still a third language.

After being reprinted once in the eighteenth century,[8] the letter was included in the four nineteenth-century editions of Galileo's works[9] as well as in a collection of his rarer writings.[10] It was also selected to be the subject of a bibliographical

compact version of this paper was read to the n International Congress of the History of Science rcelona on September 4, 1959, and the entire paper ead to the University Seminar on the Renaissance lumbia University on November 17, 1959.

iacomo Leopardi, *Operette morali*, Il Copernico, 4; *Opere* **2**: 59, Florence, 1845–1849.

e opere di Galileo Galilei **5**: 312, national edition l hereafter as "NE"), Florence, 1890–1909, re- d 1929–1939: "non solamente cattolico, ma sacerdote onico."

E **5**: 293: "non pur cattolico, ma religioso e ca- o," in a letter of February 16, 1615, to Piero Dini. : the original draft of this letter in Galileo's own riting has not survived, five manuscript copies are t (NE **5**: 270–271). First printed in 1776, the letter ncluded in the standard nineteenth- and twentieth- ry editions of Galileo's correspondence and works. lthough this allegation was promptly withdrawn by thor Galileo, it has been repeated by Benjamin Ginz- *The adventure of science*, 22, New York, 1930 ("the 1 monk, Nicholas Copernicus"), as well as by the pologist and popular psychologist George A. Dor- *Man's own show: civilization*, 675, New York and on, 1931 ("this old monk Copernicus").

[4] Edward Rosen, Galileo's misstatements about Copernicus, *Isis* **49**: 319–330, 1958; reprinted, Massachusetts Institute of Technology Publications in the Humanities **32**, 1958.

[5] NE **5**: 272–274.

[6] *Novi-antiqua . . . doctrina*, 5, Strasbourg, 1636.

[7] Thomas Salusbury, *Mathematical collections and translations* **1** (1): 429, London, 1661–1665; for new light on this elusive individual, see Jacob Zeitlin, Thomas Salusbury discovered, *Isis* **50**: 455–458, 1959.

[8] *Lettera del Signor Galileo Galilei . . . scritta alla Granduchessa*, 3, Florence, 1710.

[9] Ed. Milan, 1808–1811 **13**: 10; ed. Milan, 1832 **2** (*Biblioteca enciclopedica italiana* **21**): 376; ed. Florence, 1842–1856 **2**: 29; NE **5**: 312.

[10] Giambatista Venturi, *Memorie e lettere inedite finora o disperse di Galileo Galilei* **1**: 226, Modena, 1818–1821.

oddity, acclaimed as the smallest book in the world.[11]

In the present century the letter has appeared in the reprint of the national edition of Galileo's works,[12] in two other editions of his writings,[13] as a separate publication,[14] and in a comprehensive anthology of Italian literature,[15] besides being translated for the second time into English.[16]

All sixteen of these printed editions and translations of Galileo's *Letter to the Grand Duchess* repeated his misassignment of Copernicus to the priesthood without raising the slightest question about the matter or expressing the least doubt. Even an otherwise astute historian of Galileo's trial was equally gullible.[17]

11. THE REBIRTH OF THE MYTH

If we wish to reach the second of the three distinct historic contexts in which Copernicus was falsely labeled a priest, we must take a long leap forward from the denunciation of Copernicanism as a heresy in the seventeenth century. Two hundred years later, Copernican treatises were being removed from the Roman Catholic Index of Prohibited Books. But a new controversy concerning Copernicus had arisen between a different pair of adversaries—Germans and Poles—who bitterly disputed the astronomer's nationality instead of his orthodoxy. In the heat of this conflict between two great neighboring cultures an ardent Polish patriot, Adrian Krzyżanowski (1788–1852), emeritus professor of the University of Warsaw, giving no indication that he was aware of Galileo's misconceptions about Copernicus' status as a cleric, published the following mixture of truth and falsehood:

In Kraków after 1502, the year in which he went there from Rome, Copernicus embraced the ecclesiastical profession and was ordained there. There also, as is proved by his astronomical works, between the aforementioned year and 1509 he wrote his immortal treatise *De revolutionibus orbium coelestium*. In Kraków he found James Zaremba of Bydgoszcz, a friend of his deceased parents, still alive. During their stay in Kraków, in the year 1469 they had been admitted together with their children [18] by Zaremba as provincial to the Third Order, that is, to participation in the spiritual good works of the Dominican order of the Polish province (*provincia polona*), as we know from an original parchment which attests the affiliation of the Copernicus family with the Polish Dominicans. This Zaremba was elevated to suffragan of Kraków in 1502 by John Konarski, bishop of Kraków, and to titular bishop of Laodicea in 1503 by Pope Pius III. There are traces that Copernicus received his priestly ordination from the hands of Zaremba and Konarski.

Krzyżanowski's *Kopernik w Wathalli*, from which the foregoing excerpt is translated, was first published as a letter to a newspaper;[19] it was promptly reprinted in two literary journals,[20] and also as a separate pamphlet (Warsaw, 1843). Its diffusion beyond Polish circles was rapid. An abridgment appeared in German,[21] and a paraphrase in Russian.[22] "An address delivered at the annual meeting of the Polish Association, or-

[11] *Galileo a Madama Cristina di Lorena*, 50–51, Padua, 1896; see *La Bibliofilia* **8**: 221, 1906–1907, and **14**: 280, 1912–1913.

[12] **5**: 312, 1932.

[13] *Opere* **3**: 363–364, Florence, 1935, ed. Pietro Pagnini; *Opere* **1**: 880, Milan and Rome, 1936–1938, ed. Sebastiano Timpanaro.

[14] Galileo Galilei, *Lettera a Cristina di Lorena*, 8, Florence, 1943, ed. Giovanni Gentile.

[15] *La letteratura italiana, storia e testi* **34**:1=Galileo, *Opere*, 1010, Milan and Naples, 1953, ed. Ferdinando Flora.

[16] Stillman Drake, *Discoveries and opinions of Galileo*, 178, Garden City, 1957.

[17] Karl von Gebler, *Galileo Galilei und die Römische Curie* **1**: 80, Stuttgart, 1876–1877. The revisions made by Von Gebler for the Italian translation by Giovanni Prato, *Galileo Galilei e la Curia Romana* **1**: 86, Florence, 1879, and for the English translation by Jane Sturge, *Galileo Galilei and the Roman Curia*, 65, London, 1879, left Galileo's error intact.

[18] Krzyżanowski conveyed this detail correctly. The same cannot be said for Artur Wołyński's statement that "in order to obtain from Heaven the grace of having offspring, both spouses became tertiaries of St. Dominic, and before Father James Zaremba, provincial of the Dominicans, who enlisted them in the militia of the patriarch, they also made a vow to have their future children enrolled in the Third Order" (*Cenni biografici di Niccolò Copernico*, 9, Florence, 1873). Exactly when Copernicus' parents were married is not known, but an inheritance settlement proves that they were already man and wife in 1464 (Leopold Prowe, *Nicolaus Coppernicus* **1** (1): 66, Berlin, 1883–1884). Nine years later (not eleven, despite Prowe **1** (1): 53) the astronomer was born, the youngest of their four children; the birthdays of their three older children are unknown. Despite Wołyński, no "future children" are mentioned in the document that admitted Copernicus' parents to the Third Order in 1469. Nor did they become tertiaries "in order to obtain from Heaven the grace of having offspring."

[19] *Kurjer warszawski*, February 19, 1843.

[20] *Rozmaitości, pismo dodatkowe do Gazety lwowskiej* 1843, supplement to no. 16, with corrections suggested by someone else in no. 18: 142–143; and *Tygodnik literacki* (Poznań) **6** (23–26), 1843.

[21] Kopernik gehört *nicht* in die Walhalla, *Jahrbücher für slavische Literatur, Kunst und Wissenschaft* **1**: 247–252, 1843; our passage occurs at p. 251.

[22] Sergei Pobedonostzev, Nikolai Kopernik, *Moskvitianin*, part 5, no. 9: 119, 1843 (I wish to thank Peter Gaida of the Columbia University Library for helping me with this material).

occasion of the three hundredth anniversary the Copernican system" summarized Krzyża-ɔwski's contentions for an English-speaking ience. This address was preserved in a chure[23] offprinted from a British periodical,[24] ɔse readers were told that "Konarski and emba of Bidgosc, two high dignitaries of the rch, are named by historians as particular nds both to himself and to his family. . . . ɔernicus entered holy orders, and was conse-ed at Cracow a priest."[25] This misinforma-was soon repeated in French by one of yżanowski's compatriots whose re-broadcast, ve shall have occasion to see later on in Section in turn evoked further repercussions with lly effectiveness. All in all, there were but regions in the civilized world which did not access in their own language to Krzyżanow-asseveration that Copernicus was a priest. the struggle between Germans and Poles the nationality of Copernicus accomplished ing else, it did at least stimulate scholars in camps to undertake intensive research into ything that affected, or might have affected astronomer's career. Utilizing the positive lts of these investigations, let us try to dis-ish truth from falsehood in that curious ure quoted above from Kryzyżanowski's ıalla paper.

the first place, Copernicus is not known to been in Rome after the jubilee year 1500. ndly, he did not leave Italy in 1502, since Iay 31, 1503, he was personally awarded the ee of doctor of canon law by the University ferrara.[26] Thirdly, when he left Italy in 1503,[27] his destination was not Kraków, but Frombork (Frauenburg), the seat of the cathedral chapter of which he had become a member six years before, and which had twice granted him leave of absence to study in Italy. Fourthly, in 1507, by order of the Frombork chapter,[28] he began to attend his uncle, the bishop of Varmia (Ermland), as personal physician in the episcopal residence at Lidzbark (Heilsberg). Fifthly, he did not finish writing the *Revolutions* in 1509. Lastly, the surname of the provincial who admitted his family to the Third Order was not Zaremba. Since Krzyżanowski insisted that "Zaremba" was one of the two prelates by whom Copernicus was ordained a priest, it behooves us to scrutinize the document which enrolled Copernicus' family in the Third Order.

This document was first published in 1819.[29] After being reprinted three times,[30] it appeared in facsimile.[31] An examination of the facsimile shows that the name Zaremba did not occur in the document. In it the "Order of Preachers' provincial for Poland," who accepted Copernicus' family in the Third Order, called himself simply Friar "James of Bydgoszcz" (Jacobus de Bydgostia) without indicating his surname. Modestly describing himself as an "unworthy

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Krystyn Lach-Szyrma, *Copernicus and his native* London, 1844; translated into Polish by Erazm ɔzewski, *Kopernik i jego ojczyzna*, Gniezno, 1873. Polish version was also included in Ignacy Polkow-*Copernikijana* **2**: 149–174, Gniezno, 1873–1875, with assage at **2**: 160; moreover, at **2**: 111–119, Polkow-eprinted Krzyżanowski's *Kopernik w Walhalli.* three publications of 1873, the four-hundredth ersary of Copernicus' birth, reinforced the impact rzyżanowski's arguments on Poland thirty years they were originally advanced in 1843, the three-edth anniversary of Copernicus' death.

[24] *Foreign and Colonial Quarterly Review* **3**: 361–393, *Op. cit.*, 371; offprint, 11.

[25] His diploma was found in 1876 (twenty-four years Krzyżanowski's death), and was published by Bal-re Boncompagni, *Atti dell' Accademia pontificia de' Lincei* **30**: 341–397, 1876–1877, with an excellent copy. Maurice Daisomont mistakenly postponed nicus' acquisition of the doctoral degree until 1504 *Nicolas Copernic devant l'astronomie*, 14, Bruges,

[27] Hans Schmauch, Die Rückkehr des Koppernikus aus Italien im Jahre 1503, *Zeitschrift fur die Geschichte und Altertumskunde Ermlands* (cited hereafter as "ZE") **25**: 225–233, 1933–1935.

[28] Prowe **1** (1): 337.

[29] *Pamiętnik warszawski* **14**: 372–374, 1819. If the characters at the close of the document were correctly read as "L. S.," they undoubtedly constitute an abbreviation for *Locus Sigilli* (the place for the seal). The document itself declares toward the end: "In witness whereof, I have ordered the seal of my office as provincial to be appended to the present document"; and when it was first published, the seal was still attached to it, according to R*** [Kazimierz Lucjan Ignacy Romer], *Beiträge zur Beantwortung der Frage nach der Nationalität des Nicolaus Copernicus*, 124, Breslau, 1872. Hence, despite Henryk Baranowski, *Bibliografia kopernikowska*, no. 753, Warsaw, 1958, the letters "L. S." are not the initials of any author, and certainly not of the writer who published no. 3696 in 1953 (see Baranowski, 429).

[30] Romer, 124; Franz Hipler, *Spicilegium copernicanum*, 298, Braunsberg, 1873; Polkowski, *Kopernikijana* **3**: 6–7, with a translation into Polish at **3**: 7–8. A brief history of the document appeared in *Wick*, no. 64: 3, 1877.

[31] Artur Wołyński, *Autografi di Niccolò Copernico*, pl. XV, Florence, 1879. "The original is found in the Krasiński Library at Warsaw," reported Wołyński, who was cited by Prowe (1 (1): 313). Yet the latter, shortly after saying that the original "is now probably in St. Petersburg" (**2**: 467), remarked that it is "lost, as was pointed out in the preceding note" (**2**: 468).

professor of sacred theology," he dated the document on March 10, 1469, "in the monastery of Kraków."

This same "James of Bydgoszcz, professor of sacred theology and prior of the monastery of the Holy Trinity" had petitioned the University of Kraków on August 28, 1450[32] not only to admit the local Dominican friars to the university by incorporation but also to accept him as a teacher of theology, "and this was done."[33] Moreover, "Friar James of Bydgoszcz, master of sacred theology, who ruled the province of Poland for twenty-seven years," died in 1478, according to the necrology of the Dominican monastery of the Holy Trinity at Kraków.[34]

Since James of Bydgoszcz, who made Copernicus' family Dominican tertiaries in 1469, died in 1478, he obviously could not, despite Krzyżanowski, have ordained Copernicus a priest after 1502. Nor, again despite Krzyżanowski, could James of Bydgoszcz have been "elevated to suffragan of Kraków in 1502 by John Konarski, bishop of Kraków,[35] and to titular bishop of Laodicea in 1503."[36] In those early years of the sixteenth century the suffragan of Kraków and titular bishop of Laodicea was John Przyjaciel of Kraków, who latinized his surname as "Amicinus."[37]

Furthermore, the surname of the provincial who presided over the initiation of Copernicus' family into the Third Order is transmitted to us by one of his contemporaries, Jan Długosz (1415–1480). This famous Polish historian recorded the surname in an essay which remained in manuscript until twelve years after Krzyżanowski's death. Then in 1864 one of the volumes in Długosz's collected works appeared, containing his list of the "Names of the provincials of the Order of Preachers in the kingdom of Poland and its province." There we read that "Friar James of Bydgoszcz, professor of sacred theology, by birth a noble of the house of Godzienba,[38] prior of Kraków, was unanimously elected in a general

[32] Four years earlier, on August 22, 1446, James of Bydgoszcz as prior and the other friars of the monastery of the Holy Trinity in Kraków sold a house to the municipal authorities; see *Codex diplomaticus civitatis cracoviensis 1257–1506* **1**: 208, Kraków, 1879–1882, ed. Franciszek Piekosiński.

[33] *Conclusiones universitatis cracoviensis ab anno 1441 ad annum 1589*, 15, Kraków, 1933, ed. Henryk Barycz.

[34] Heinrich Zeissberg, Kleinere Geschichtsquellen Polens im Mittelalter, *Archiv für österreichische Geschichte* **55**: 150, 1877; the necrology records the death of James of Bydgoszcz as having occurred on May 30, 1478.

[35] The anachronistic idea that John Konarski made James of Bydgoszcz his suffragan appeared as early as Severinus, *De vita . . . Hyacinthi*, 54, Rome, 1594. But after James of Bydgoszcz died in 1478, a quarter of a century elapsed before John Konarski became bishop of Kraków in 1503, as was pointed out by Sadok Barącz, *Rys dziejów zakonu kaznodzicjskiego w Polsce* **2**: 50, Lvov, 1861.

[36] James of Bydgoszcz was in fact named titular bishop of Laodicea, but not in 1503. *Hierarchia catholica medii et recentioris aevi* **2**: 190, Munster and Padua, 1901–1958, includes in its list of titular bishops of Laodicea in Syria "Jacobus de Bydgoszcza, O. Praed. 1481." While *Hierarchia catholica*'s date (1481) for the elevation of James of Bydgoszcz to the Laodicean bishopric is not as inaccurate as Krzyżanowski's date (1503), both dates must be belated since the bishop died in 1478 (see n. 34, above). He owed his double elevation to Jan Rzeszowski, who was bishop of Kraków from 1471 to 1488; see Barącz **2**: 50, and Szymon Starowolski, *Vitae antistitum cracoviensium*, 181–183, Kraków, 1655.

[37] Jan Nepomucen Fijałek (1864–1936), Studya do dziejów uniwersytetu krakowskiego i jego wydziału teologicznego w XV wieku, *Rozprawy akademii umiejętności, wydział filologiczny*, Kraków, ser. 2 **14**: 17, 1898. Amicinus was suffragan of Kraków as early as December 29, 1501 (*Conclusiones univ. cracov.*, 95). Again belated, *Hierarchia catholica* (**2**: 311) dates Amicinus' appointment as suffragan of Kraków in 1503; the month and day must be August 9 or earlier, since it records his elevation to the bishopric of Laodicea as follows: "Joannes (suffr. epi. Cracovien.) 1503 Aug. 9" (**2**: 190). For Amicinus' retention of both these titles as late as 1524, see *Catalogus diplomatum pergamencorum universitatis jagellonicae cracoviensis*, p. 192, no. 394, Kraków, 1953, ed. Kazimierz Kaczmarczyk. Amicinus died on October 16, 1526 (*Acta rectoralia almae universitatis studii cracoviensis* **1**: 726, Kraków, 1893–1909, edd. Władysław Wisłocki and Stanisław Estreicher).

[38] The credit for correcting Krzyżanowski's substitution of "Zaremba" for James of Bydgoszcz's true surname Godziemba should be given to Ludwik Antoni Birkenmajer, *Mikołaj Kopernik*, 432, Kraków, 1900. Yet strangely enough, L. A. Birkenmajer took a less decisive position on p. 572. There, instead of rejecting the pretended surname Zaremba outright, he merely termed it "at least questionable," and said only that Fijałek called James of Bydgoszcz "Godziemba." But this question of the surname is not to be decided by citing a nineteenth-century scholar who cries "Godziemba" against another who shouts "Zaremba." Actually Fijałek found the true surname Godziemba in Długosz. However defective the Renaissance historian's accounts of remote times and places may have been, he certainly can be trusted when he is discussing his own contemporaries and his own immediate vicinity. His "Names of the provincials of the Order of Preachers in the kingdom of Poland" was to extend, he explained, from the first such provincial to the modern, namely, Friar James of Bydgoszcz," who was prior of the Dominican monastery in Kraków when Długosz was a canon of the cathedral chapter in the same city. The misassignment of James of Bydgoszcz to the Zaremba family did not originate with Krzyżanowski; see n. 50, below.

pter held in the monastery at Sieradz in the r 144.;[39] becoming paralyzed at Kraków in '8, he died on May 3[40] and was buried in the nastery of the Holy Trinity of the Order of achers."[41]

As a matter of sober historical fact, then, it was nes Godziemba (not Zaremba) of Bydgoszcz o inducted Copernicus' family into the Third der in 1469. Godziemba was later designated llar bishop of Laodicea, apparently shortly ore his death in 1478. He had no part in any ged ordination of Copernicus as a priest after 12.

Not content to offer a bare assertion like Gali- s, Krzyżanowski adorned his fantasy with a cific place (Kraków), an approximate time ter 1502), and the names of two presiding lates (Zaremba and Konarski). In venturing supply such definite details Krzyżanowski per- s imagined that he was enhancing his credi- ty. Instead these very details turned out to his Achilles' heel, since they permitted him to refuted by lovers of the truth using unimpeach- e sources, the publication of which he did not esee.

Nevertheless, Krzyżanowski himself may have sed the weakness of his Valhalla paper. For returned to the fray three months later in his *mnienie jubileuszowe*[42] (or Commemoration Memoir, as we may call it), which he dated May 24, 1843, the tercentenary of Copernicus' death. In his Commemoration Memoir he said about the astronomer: "In Kraków after 1502, the year in which he returned there from Rome, he embraced the ecclesiastical profession and received his ordination there."[43] The multiple errors stuffed into this one sentence have been disclosed above in our analysis of Krzyżanowski's Valhalla paper. But now in his Commemoration Memoir he proceeded to add the following new material:

> This is the understanding of Samuel Luther Geret, doctor of law, court secretary of King Poniatowski in the administration of the city of Toruń, in his manuscripts known to us concerning Copernicus, and this follows from the fact that after 1509, when Copernicus left Kraków for Varmia, he was already a priest.[44]

The Commemoration Memoir's statement that Copernicus was already a priest in 1509 when he left Kraków is not really new. For the Valhalla paper had previously maintained that the astronomer lived in Kraków from 1502 to 1509, and that Kraków was the scene of his ordination as a priest after 1502 (and therefore before 1509). Thus the Commemoration Memoir's unsupported assertion that Copernicus became a priest at the latest in 1509 adds absolutely nothing to the Valhalla paper's unsupported assertion that he was ordained after 1502.

Hence the only really new element for us in the Commemoration Memoir is its mention of references to Copernicus in the manuscripts of Geret (1730–1797). Born in Toruń (Thorn), Copernicus' birthplace, Geret filled various municipal offices of his native city, and also edited a local weekly newspaper for twelve years. Shortly before his death he published two books about his home town, one focusing on its contemporary situation and the other on its historical past. He was appointed to several royal posts by his ruler, King Stanisław Poniatowski, at whose court in Warsaw he resided for some time. Living in the period when Poland, once a major sovereign state, was sinking below the political horizon while the star of Prussia's might was rising, Geret tried to instill nationalistic feeling in the Prussians inhabiting Poland; he called "the

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The last digit is missing in Długosz, who reports Godziemba's predecessor died in 1441. According ie necrology cited in n. 34 above, James of Bydgoszcz "ruled the province of Poland for twenty-seven s" at the time of his death in 1478; if so, he took e soon after 1450. Why did Barącz (2: 50) give date as not later than 1447? James of Bydgoszcz amed as the provincial in 1454 (*Codex diplomaticus atis cracoviensis* 1: 222).

According to the necrology cited in n. 34 above, es of Bydgoszcz died on May 30.

Długosz, *Opera omnia* 9: 453, Kraków, 1863–1887 = *r beneficiorum dioecesis cracoviensis* 3: 453; see 49 for the passage quoted toward the end of n. 38, c.

This "Memoir in commemoration of Nicholas Coper- s, the founder of modern astronomy, in the three- lredth year since his death and the promulgation of system" was published separately (Warsaw, 1844), also constituted pp. 1–34 of the appendix to Krzyżan- ci's *Dawna polska* (Warsaw, 1844), a book on early nd and its contribution to human progress. The memoration Memoir was promptly reprinted twice: *ndar Niezabitowskiego*, 1844, and *Przyjaciel ludu* szno) 11: pt. 1, no. 4–8, 1844. Five years after yżanowski's death, his *Dawna polska* was brought n a second edition (Warsaw, 1857) by Hipolit Skim- wicz, who included the Commemoration Memoir in : 99–132, as did also Polkowski in his *Kopernikijana* 2: 127–147. I wish to thank Wanda Borkowska, librarian of the Bibliothèque Polonaise in Paris, for her courtesy in making available to me the 2d ed. of *Dawna polska*.

[43] *Spomnienie jubileuszowe*, 23; *Dawna polska*, appendix, 23; 2d ed., pt. 1: 122; Polkowski 2: 140.

[44] *Ibid.*

Poles the enemies of Prussia."[45] He would hardly have sympathized with Krzyżanowski's thesis that Copernicus was a Pole.

What do Geret's manuscripts say concerning Copernicus? In all the vast literature that has been published about the astronomer in the 115 years since Krzyżanowski's extremely vague citation of these manuscripts[46] first appeared in print (his citation has been republished four times), they have been discussed only once, and that unique discussion is openly skeptical.[47] Therefore, while awaiting more precise information on these manuscript references to Copernicus, I should be inclined to recommend suspension of judgment about Geret's "understanding," as relayed to us by Krzyżanowski.

In the meantime it is possible to examine one instance of Krzyżanowski's use of a manuscript. Długosz's "Names of the provincials of the Order of Preachers in the kingdom of Poland and its province" was still only an unpublished manuscript when Krzyżanowski's Commemoration Memoir was printed. With regard to James of Bydzoszcz, as we saw above,[48] Długosz's manuscript reports that his surname was Godziemba, that he was elected in the general chapter held at Sieradz in the decade of the 1440's, and that he died in 1478. But Krzyżanowski's Commemoration Memoir airily declared that "James Zaremba of Bydgoszcz . . . was elected by a meeting of the chapter at Sieradz in 1478."[49] Why did Krzyżanowski accept the transfer of James of Bydgoszcz from one noble Polish family to another? Why did he accept the transformation of the date of James of Bydgoszcz's death into the date of his election as provincial?[50] Was it because he wanted to pretend that the provincial who made the astronomer's parents Dominican tertiaries in 1469 lived long enough to ordain Copernicus as a priest between 1502 and 1509? For Krzyżanowski's Commemoration Memoir continued as follows:

> After serving the Order in this office [of provincial] for twenty-four years, in 1502 Zaremba was elevated to suffragan of Kraków by John Konarski, bishop of Kraków, and in 1503 to titular bishop of Laodicea by Pope Pius III. There are indications that Copernicus received his priestly ordination at the hands of Zaremba and Konarski.[51]

The nature of these "indications" can readily be surmised by witnesses of Krzyżanowski's deliberate distortions of Długosz's manuscript. No wonder Krzyżanowski's principal German antagonist called him an "unreliable writer." Even a staunch Polish admirer of Krzyżanowski, describing him as hot-headed, hasty, and prone, spoke of his "numerous blunders and many untenable, exaggerated assertions."[52]

III. THE SPREAD OF THE MYTH

Despite such shortcomings Krzyżanowski exerted an enormous influence both inside and outside. We have already seen[53] how dubious doctrines were propagated in German, Russian, and English. But by far his greatest echo was Jan Czyński (1801–1867),[54] the author of *Kopernik et ses travaux*, Paris, 1847. Even after most Frenchmen writing about Coper-

[45] S. L. Geret, *Die aus den Gräbern durchdringende Stimme derer vor zwey hundert und hundert funfzig Jahren verstorbenen wahren und ächten Preussen, zur Erweckung und Besserung an die jetzt lebenden zu Polen ausgearteten Preussen*, 6, Mitau, 1774; Geret begins this book, which was published anonymously for obvious reasons, with the lament "that we have become Poles and have ceased to be Prussians" (p. 5).

[46] R*** [Romer], 169, was impressed by Krzyżanowski's "explicit" citation of Geret's manuscripts. But would not an additional indication of the exact passage have made Krzyżanowski's citation even more explicit, thereby helping to settle the question whether he was writing history or creating mystery?

[47] Gdzie jest Mandatum regium pro administratore Copernico w rękopisie Gereta?, *Wiek*, 1887, no. 205.

[48] In the text at n. 41.

[49] *Spomnienie jubileuszowe*, 24; *Dawna polska*, appendix, 24; 2d ed., pt. 1: 122; Polkowski **2**: 141.

[50] The misassignment of James of Bydgoszcz to the Zaremba family appears as early as Severinus . . . *Hyacinthi*, 54), who also transformed 1478 . . . year in which James of Bydgoszcz was elected provincial, even though at p. 66 Severinus dated the . . . James of Bydgoszcz in 1477.

[51] *Spomnienie jubileuszowe*, 24; *Dawna polska*, appendix, 24; 2d ed., pt. 1: 122; Polkowski **2**: 141. After Krzyżanowski's "Zaremba" never existed. John Konarski (1447–1525) actually was the bishop of Kraków for more than twenty-one years from 1503 until his death on May 3, 1525; see Starowolski, *Vitae antistitum cracoviensium*, 191–193, or Ludwik Lętowski, *Katalog biskupów, opatów i kanoników krakowskich* **2**: 58-73, Kraków, 1853.

[52] Prowe 1 (1): 48. Prowe's general distrust of Krzyżanowski did not save him from being deceived by his adversary's interpolation of "Zaremba" in the Długosz document, which he reprinted (**2**: 468).

[53] R*** [Romer], 111, 141, 162.

[54] In the text at nn. 21–25, above.

[55] For a brief sketch of Czyński's life and character, see Stanisław Wędkiewicz, *Etudes coperniciennes* 1: 1–2, Paris, 1955–1957.

v much of their information (and misinforma-) about him from Czyński. Thus at three ts in Czyński's extensive Table of Contents could read that Copernicus "becomes a st," "embraces the ecclesiastical condition ugh love of science," and "becomes a priest rofession, fulfilling all its duties with remark- punctiliousness."[56] This last unhistorical luct of a devout imagination was expanded Czyński's Introduction,[57] which located in ków Copernicus' "embracing of the ecclesi- al condition as his vocation."[58] For this ositious action Czyński's text generously ished Copernicus' motive:

oped to find an unknown route, to discover the mechanism of the universe, and to give a new dse to the investigations pursued by the human l. In order to attain this end, he had to have a eful refuge and a life free from storms. The siastical condition corresponded best to his inner ig and future plans. Guided as much by piety y the desire to continue his astronomical re- ches, he renounced the life of the world and me a priest. James Zaremba, suffragan, and Konarski, bishop of Kraków, conferred holy rs on him.[59]

ing provided the astronomer with a praise- hy purpose in picking the profession of priest, iski proceeded to describe Copernicus' at- le toward the career which he actually did choose: "He wished to fulfill his duties as st, giving proof of great piety and bringing h precision to his performance of the divine ion."[60] "Copernicus' scientific work did not ent him from carrying out his duties as t and as a citizen."[61] "Copernicus' be- or . . . provides an example which ought to llowed by the clergy of all countries. His ion as a priest did not make him forget his ations as a citizen."[62] Czyński climaxed ccount of Copernicus' twofold services by ing on the astronomer the hyphenated label st-citizen."[63]

n entire chapter[64] of Czyński's book was de- l to Krzyżanowski's Valhalla paper. From zyński translated a sentence with which we have already become familiar: "Upon his return to Kraków, Copernicus embraced the ecclesiastical profession, and it was at Kraków that he was ordained."[65] After correcting an error committed by Krzyżanowski, Czyński continued:

Here the learned professor enters into minute details concerning the personal life of Copernicus. At Kraków he finds James Zaremba, who lets him share the rights enjoyed by the Dominicans in Poland. It was this bishop, as well as Konarski, bishop of Kraków, who presided at Copernicus' ordination.[66]

By attributing to Krzyżanowski the statement that Copernicus joined the Dominican Third Order, Czyński imputed to his compatriot a blunder which Krzyżanowski did not in fact make. For in his Valhalla paper, as we saw above,[67] Krzyżanowski listed the persons admitted to the Third Order as Copernicus' parents "together with their children." But these children did not include the future astronomer. He was born in 1473, nearly four years after his parents entered the Third Order, taking with them those of their children who had been born before March 10, 1469. To enlist Copernicus among the Dominican tertiaries four years before his birth would have involved an absurd anachronism of which Krzyżanowski was not guilty. He neither stated nor implied that Copernicus became a Dominican tertiary. It was Czyński who enrolled the astronomer in the Third Order; and he placed that event (which never occurred) more than thirty years after the date of the document attesting the admission into the Third Order of Copernicus' family without the astronomer.

Close attention to chronology, however, was not conspicuous among Czyński's virtues. Thus he put Copernicus' researches "in the middle of the fifteenth century,"[68] although that century's third quarter was nearly over at the time of the astronomer's birth in 1473, a date which Czyński knew perfectly well.[69] While aware that Copernicus died seventy years later in 1543,[70] Czyński nevertheless computed that the astronomer's "life ended at the age of seventy-three."[71] Copernicus' observation of an occultation of Aldebaran on March 9 was assigned by Czyński to 1496[72] in-

[56] p. xiv, viii, iii.
Dated August 10, 1845 (p. 19); the Dedication is September 10, 1846 (p. iii).
[58] p. 12.
[59] p. 34–35.
[60] p. 35.
[61] p. 44.
[62] p. 54.
[63] p. 83.
[64] Ch. XIV, pp. 174–192.

[65] P. 188.
[66] P. 189.
[67] In the text at n. 18.
[68] P. 14; see also p. 172.
[69] P. 26.
[70] P. 84.
[71] P. 85.
[72] P. 32.

stead of 1497.[73] In the dedication of the *Revolutions* Copernicus declared that he had kept his book away from publishers "not only nine years but four times nine years"; that interval was mistranslated by Czyński as "more than twenty-seven years."[74] The first printed explanation of the Copernican theory was written in 1539 by Copernicus' disciple, George Joachim Rheticus, who dated it "on the ninth day before the calends of October"; this day in the calendar of ancient Rome was misequated by Czyński[75] with October 9 (instead of September 23).[76] He called Tycho Brahe (1546–1601) a contemporary of Copernicus,[77] even though the great Danish observer was born more than three years after Copernicus died. Jan Brożek (1585–1652)[78] was converted into a "friend and confidant" of Copernicus by Czyński,[79] despite the latter's quotation[80] of Krzyżanowski's correct description of Brożek as "professor of astronomy in Kraków at the beginning of the seventeenth century." Having noted[81] that Galileo's *Dialogue* was published in 1632, Czyński remarked: "it is on account of this book" that the eminent Italian scientist "was summoned before the tribunal of the Sacred Congregation of the Cardinals, under Paul V, in 1610."[82]

Pierre Gassend (1592–1655), the famous French philosopher whose life of Copernicus was published in 1654, was called by Czyński "the first biographer of Copernicus"[83] and "the only biographer of Copernicus,"[84] even though Czyński himself acknowledged that "a short biography of Copernicus,"[85] "two pages"[86] in length, had previously appeared in the 1617 edition of the *Revolutions*.[87] Erasmus Reinhold's *Prussian tables* (Tübingen, 1551) gave Czyński particular trouble. In one passage[88] he attributes them to Copernicus who, he says, sent them to Rome (presumably before 1543); in another passage he names Rheticus as "the author of the *Prussian tables*."[89] Copernicus' reference to the discovery of America was displaced by Czyński from Book I, chapter 3, of the *Revolutions* to "the first chapter."[90] In describing Copernicus' living quarters, Czyński remarked that "the simple astronomical instruments were made with his own hands."[91] Yet previously Czyński had said that "the only instrument which Copernicus had at his disposal"[92] was the parallactic instrument. Moreover, Czyński added that "we see in the papers of Tycho Brahe, found after his death, the description of the parallactic instrument made by Copernicus' hand and used by him in his observations."[93] This formulation indicates that Czyński was unfamiliar with Copernicus' detailed description of the parallactic instrument in the *Revolutions*.[94] By saying that Copernicus had only the parallactic instrument, used mainly for observing the moon, Czyński showed that he was equally unfamiliar with Copernicus' descriptions

[73] *Revolutions*, IV, 27.

[74] P. 72; *cf.* p. 185.

[75] P. 62.

[76] Edward Rosen, 196, *Three Copernican treatises*, New York, 1939; 2d ed., Dover Publications, 1959.

[77] P. 103.

[78] For a brief account of this enthusiastic admirer of Copernicus, together with a useful list of publications about Brożek, see Wędkiewicz, *Etudes copernicaniennes* **1**: 7–11.

[79] P. 96.

[80] P. 182.

[81] P. 252.

[82] P. 258.

[83] P. 27.

[84] P. 120; when he called Gassend "the only serious biographer" of Copernicus, Czyński ignored Georg Christoph Lichtenberg (1797, 1803) and Johann Heinrich Westphal (1822).

[85] P. 168.

[86] P. 169.

[87] This in turn had been preceded by the biography of Copernicus in Melchior Adam, *Vitae Germanorum . . . clarorum*, 126–128, Frankfurt, 1615; this work was issued also at Heidelberg in 1615 under the title *Vitae Germanorum philosophorum*. Besides these two biographies of 1615 and 1617, a third biography of Copernicus was published before Gassend's. In fact, this third biography appeared in two quite different versions: one in the first edition of Szymon Starowolski's *Scriptorum Polonorum . . . vitae* (Frankfurt, 1625; reprinted 1644), and the other in the second edition (Venice, 1627). If it is true that Starowolski's second version was actually written by Brożek, the latter is still not Copernicus' "first biographer," despite Wędkiewicz, 10; nor is the 1617 biography the "first biography of Copernicus" (Wędkiewicz, 9), since it was preceded by Adam's. Some people will doubtless enjoy being assured that Adam came first. For the question whether Brożek wrote the second version of Starowolski's biography of Copernicus, see Aleksander Birkenmajer, Czy Leowitz był przeciwnikiem Kopernika?, *Kwartalnik historii nauki i techniki* **4**: 20–21, 1959 (summary in English at pp. 32–33).

[88] P. 57.

[89] P. 97.

[90] P. 37. Edward Rosen, Copernicus and the discovery of America, *Hispanic American Historical Review* **23**: 367–371, 1943.

[91] P. 95.

[92] P. 39.

[93] P. 38.

[94] Bk. IV, ch. 15.

the quadrant,[95] used for the sun, and of the illary sphere,[96] used for the stars.[97] Coper-is' proposals for reforming the Prussian cur-cy would have abolished the coining of money the three cities which still possessed that rilege; but Leipzig was not one of those three es, despite Czyński,[98] who undoubtedly meant izig.[99]

His acquaintance with the history of astronomy somewhat less than complete. When he tried tell his readers how highly Copernicus was rded by Christopher Clavius (1538–1612), ński made some comical slips. Clavius' most ular publication, which was issued over and r again, was cast in the form of a commentary thirteenth-century textbook by Sacrobosco.[100] licrously inverting the relationship of these men, Czyński has Socro Bosco (as he calls) "reproducing the very words of Flavius"[101] he calls Clavius). He laments "the fate of gil,[102] bishop of Salzburg, condemned as a tic because he was the first man who dared proclaim the sphericity of the earth and the tence of antipodes." Yet Czyński himself hasized that Copernicus "recalls Lactantius,[103] poked fun at people convinced that the earth ound," "Lactantius,[104] a caustic writer who uled the mathematicians desiring to prove sphericity of the earth." And Czyński ed[105] Kepler's reference to "Augustine who, e admitting the roundness of the earth, was illing to believe in the antipodes." Could ński possibly have been unaware that the war ed by the church fathers Lactantius and Saint ustine against the correct conceptions of a d earth inhabited by antipodes was fought he fourth and fifth centuries, hundreds of s before Vergil of Salzburg was condemned by the Pope in 748?[106] Could Czyński have been so utterly ignorant of ancient Greek literature as to have been serious in asserting that Copernicus' discoveries "gave us the first idea of the infinite"?[107] Czyński was in deadly earnest when he mistook[108] that powerful woman, Sophocles' Electra, for "an electric power."[109]

Copernicus' friend, Tiedemann Giese, bishop of Chełmno (Kulm), was not an archbishop, nor was he referred to as a cardinal in the third edition of the *Revolutions*, despite Czyński.[110] Francesco Maurolico (1494–1575), in whose judgment Copernicus deserved a whipping rather than a refutation,[111] was not a Jesuit, despite Czyński,[112] but the abbot[113] of a Benedictine monastery near Castelbuono.[114] Kepler's mendacious announcement that he "embraced and approved Catholic doctrine" was contained, not in a letter to Galileo (despite Czyński),[115] but in the German's "Advice to foreign, especially Italian, booksellers."[116]

While being interrogated by the Inquisition about his *Dialogue*, Galileo pleaded (says Czyński)[117] that he had taken "good care not to mention the name of Kepler, whose works offered much stronger arguments [than the *Dialogue's*]

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evolutions, Bk. II, ch. 2.

ev., Bk. II, ch. 14.

adeusz Przypkowski, Les instruments astrono-s de Nicolas Copernic, *Archives internationales oire des sciences* **6**: 220–226, 1953.

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ean-Yves Le Branchu, *Ecrits notables sur la mon-* : 28, Paris, 1934.

Lynn Thorndike, *The* Sphere *of Sacrobosco and umentators*, Chicago, 1949.

P. 57.

Whom he calls "Vigilius" here (p. 218) and "Vir-" (p. 262).

Called "Jactance" by Czyński (p. viii).

'Jactance" again (p. 37).

P. 241, where Czyński finally reproduced the name antius" properly.

[106] *Die Briefe des h. Bonifatius*, 178–179, Berlin, 1916, ed. Michael Tangl (*Epistolae selectae . . . ex Monumentis Germaniae historicis* **1**). See John Louis Emil Dreyer, *History of the planetary systems from Thales to Kepler*, 224–225, Cambridge, England, 1906 (reprinted by Dover Publications, New York, 1953), and Hermann Krabbo, Bischof Virgil von Salzburg und seine kosmologischen Ideen, *Mitteilungen des Instituts für oesterreichische Geschichtsforschung* **24**: 1–28, 1903. An unconvincing argument that Vergil of Salzburg was neither an antipodist nor a bishop was concocted by Francis S. Betten, *St. Boniface and St. Virgil*, 36–39, 60–63, Washington, D. C., 1927 (Benedictine Historical Monographs **2**).

[107] P. 14.

[108] P. 102.

[109] As was pointed out by Alexander von Humboldt, *Kosmos* **2**: 500, Stuttgart, 1845–1862; *cf. Correspondance d' Alexandre de Humboldt avec François Arago (1809–1853)*, 289, Paris, 1908, ed. Ernest-Théodore Hamy.

[110] Pp. 71, 170; Czyński dated the third ed. of the *Revolutions*, which actually appeared in 1617, in 1807 (p. xiii) and 1607 (p. 168).

[111] Edward Rosen, Maurolico's attitude toward Copernicus, *Proc. Amer. Philos. Soc.* **101**: 177–194, 1957.

[112] P. 11.

[113] Edward Rosen, Maurolico was an abbot, *Archives internationales d'histoire des sciences* **9**: 349–350, 1956.

[114] Lawrence Henry Cottineau, *Répertoire topo-bibliographique des abbayes et prieurés*, 618, Mâcon, 1935–1938.

[115] P. 249.

[116] Johannes Kepler, *Gesammelte Werke* **6**: 543, Munich, 1940.

[117] Pp. 262–263.

in favor of Copernicus." Czyński takes good care not to mention the source of his statement. Actually Galileo mentioned the name of Kepler twenty times in his *Dialogue*, but shortly after its publication he wrote to a correspondent that "some of Kepler's ideas tend to weaken rather than strengthen Copernicus' theory." [118] To support his story that Galileo, in abjuring Copernicanism after being sentenced to prison by the Inquisition, "with his foot tapped the earth and said in a low voice 'it still moves,' " [119] Czyński resorted to the vague formula "we are assured that. . . ." [120] In quoting Copernicus as having predicted [121] to his critics that they would see the phases of Venus "if you find a means of perfecting your sight," [122] Czyński declared that "tradition has preserved for us some expressions which Copernicus used in defending his principles." [123]

But tradition was not what Czyński emphasized when he claimed "to have found documents which had escaped the researches of the scholars," [124] and to have "based on irrefutable proofs" [125] his account of Copernicus' ancestry. With regard to Krzyżanowski's insistence that Copernicus developed the heliocentric system while he was still a student at Kraków, Czyński commented: "We do not even know on what Krzyżanowski could base this assertion. He found no trace of it in the *Revolutions*, no trace in the printed works and manuscripts of [Copernicus'] Italian professors"; [126] the assertion "is not in conformity with Copernicus' own words." [127] Krzyżanowski's assertion regarding the time and place of Copernicus' discovery was rejected by Czyński, who demanded a printed or manuscript source. Yet he made no such demand in connection with Krzyżanowski's assertion that Copernicus was a priest.

Krzyżanowski upbraided his German opponents for not distinguishing between history and mythology, between prose and poetic fiction, between truth and falsehood.[128]. Since he seems not to have known about Galileo's declaration that Copernicus was a priest, the question naturally arises whether Krzyżanowski, like Galileo, fabricated the fable of Copernicus' priesthood in the forge of his own superheated imagination or perhaps took it from some source other than Galileo.

Although I cannot answer this tantalizing question definitively, I should like to propose a possible solution. Nearly a decade before Krzyżanowski wrote his Valhalla paper and Commemoration Memoir, a short story entitled "The youth of Copernicus" appeared in a Warsaw literary magazine.[129] The authoress of this frankly fictional piece drew a romantic picture of Copernicus as a young student on his way home from the university

[118] NE **14**: 340.

[119] This "eppur si muove" legend was invented by the litterateur and lexicographer Giuseppe Baretti (1719–1798) in his *Italian library*, 52, London, 1757. As a myth-maker, however, Baretti had enough historical sense to put the fictitious words in Galileo's mouth, not when he abjured, but "the moment he was set at liberty." The precise moment at which the fictitious words were declared to have been spoken became an issue of supreme importance for William Whewell (1794–1866), Master of Trinity College, Cambridge. In 1847, the year in which Czyński's *Kopernik* was published, Whewell issued a revised edition of his *History of the inductive sciences*. In this second edition (1: 464) Whewell made a remark (which has since been quoted with glee by Roman Catholic detractors of Galileo) that he did not "see with what propriety Galileo can be looked upon as a 'martyr of science'. . . . He would really have acted as a martyr, if he had uttered his 'e pur si muove' in the place of his abjuration, not after it." In its original form, which so signally failed to satisfy Whewell's strict standards for qualification as a martyr, Baretti's legend was translated from English into French in *Querelles littéraires* **3**: 49, Paris, 1761. The author of this anonymously published work was the Abbé Simon-Augustin Irail (1719–1794). His surname was transformed into "Trailh" by Hermann Kesten, *Copernicus und seine Welt*, 435, 509, Amsterdam, 1948 (first printed in English translation under the title *Copernicus and his world*, 302, New York and London, 1945, 1946).

[120] P. 272.

[121] P. 251.

[122] P. 101.

[123] P. 100.

[124] P. 14.

[125] P. 26.

[126] Pp. 184–185.

[127] P. 186. But Albert Caprimus, dedicating a book to Bishop Samuel Maciejowski on September 27, 1542, in Kraków, referred to "Nicholas Copernicus, canon of Varmia, who once enjoyed the hospitality of this city and first drew from our university, as from a spring, those marvelous things which he has written on mathematical subjects as well as the further material which he has begun to publish. This he not only does not deny in agreement with Pliny's judgment that to name those from whom we have benefited is an act of courtesy and thoroughly honest modesty) but whatever the better he says that he received it all from our university" (Warsaw, 1854, ed. of the *Revolutions*, 642).

[128] *Spomnienie jubileuszowe*, 31; *Dawna polska*, appendix, 31; Polkowski **2**: 146.

[129] Anna Nakwaska (1781–1851), Młodość Kopernika. *Jutrzenka*, 1834, pp. 209–255; reprinted in Polkowski, *Kopernikijana* **3**: 342–358, and in *Od naszego wieku* **1**: 1923, no. 4, pp. 116–129; translated into French by Olympe Chodźko under the title "La jeunesse de Copernik" in *La Pologne historique, littéraire, monumentale et illustrée*, 227–234, Paris, 1839–1841, ed. Léonard Chodźko (reprinted in *Revue des feuilletons* **4**: 283–294, 1844).

rescuing a fair princess in distress, who later visited him after he had become a priest.[130] Was it perhaps from this highly imaginative narrative that Krzyżanowski derived his entirely unhistorical conception of Copernicus as a priest?[131] While Krzyżanowski did not refer to the short story, it was summarized by Czyński for his readers. His brief summary contained the statement that "Copernicus embraced the ecclesiastical profession," after which he added: "We do not know whether this little novel has any historical basis."[132]

Historical basis or not, Czyński's statement that Copernicus was a priest was promptly repeated by highly influential French writers. Thus the permanent secretary of the French Academy of Science, François Arago (1786–1853), asserted in his biography of Copernicus that "on his return to Kraków in 1502, he became a priest."[133] The same assertion was made by Ferdinand Hoefer (1811–1878)[134] in the article which he wrote on Copernicus in his widely consulted *Nouvelle biographie générale*.[135] Louis Figuier (1819–1894), whose numerous books and plays did so much to bring the achievements of science and technology to the attention of the general public, declared that Copernicus

> preferred to renounce the life of the world and to embrace the ecclesiastical profession. The bishop of Kraków, John Konarski, and the suffragan, James Zaremba, conferred holy orders on him. It was for the sake of devoting himself entirely to the sciences that he embraced the ecclesiastical profession. From the time that he became a priest. . . .[136]

Copernicus became a priest also in the world-famous lexicon of Pierre Larousse (1817–1875).[137] The distinguished astronomer and popularizer of that science, Camille Flammarion (1842–1925), published a biography of Copernicus in which the first topic listed for discussion in Chapter IV reads: "He becomes a priest upon his return to Kraków"; [138] then Chapter IV recounts that Copernicus "became a priest. John Konarski, bishop of Kraków, conferred holy orders on him. A few years later, in 1510. . . ."[139] A standard encyclopedia professed to be more precise by saying that Copernicus "returned to Kraków at the end

[130] Polkowski **3**: 353, 356–357; *La Pologne*, 232–233.

[131] In 1820 Krzyżanowski wrote an "Essay on the life of the Polish astronomer Nicholas Copernicus" in French. The autograph manuscript of this *Notice*, which apparently was never printed, was later offered for sale (*Przewodnik bibliograficzny* **11**: 119, 1888). If it is still extant, an examination of its contents may reveal whether it described Copernicus as a priest fourteen years before Krzyżanowski could have read Nakwaska's short story. The *Notice* appears to be the fulfillment of a promise made by Krzyżanowski in a letter dated December 20, 1819, and published by Aleksander Kraushar, *Towarzystwo królewskie przyjaciół nauk* **3** (1): 394–398, Warsaw and Kraków, 1900–1911 (the citation in Baranowski, *Bibliografia kopernikowska*, 135, should be changed from **4** to **3**); at p. 395, where Copernicus' ecclesiastical career is described, Krzyżanowski says nothing about his having been a priest. Readers who would like to know what Krzyżanowski looked like will find a photograph of him as the frontispiece of the 2d ed. of his *Dawna polska* as well as in Kraushar **3** (1): 345.

[132] Pp. 313–314.

[133] Arago, *Œuvres complètes* **3**: 175, Paris and Leipzig, 1854–1862; at **3**: 173 Czyński is cited. Arago's complete works were immediately translated into German under the title *Sämmtliche Werke*, Leipzig, 1854–1860, by Wilhelm Gottlieb Hankel, who challenged some of Arago's statements, but not the one which maintained that Copernicus was a priest (**3**: 139).

[134] For a deeply moving analysis of his character and publications, see George Sarton, Hoefer and Chevreul, *Bulletin of the History of Medicine* **8**: 420–437, 444–445, 1940.

[135] **28**: 60, Paris, 1859; at **28**: 74 Hoefer cited Czyński's book (which he misdated in 1846). In his *Histoire de l'astronomie*, 294, Paris, 1873, Hoefer reiterated his misstatement about Copernicus' priesthood.

[136] *Vie des savants illustres* **3**: 363–364, Paris, 1868; repeated in 2d ed., 1875, and 3d ed., 1881, as well as in the translation into Spanish by Pelegrín Casabó y Pagés, *La ciencia y sus hombres* **2**: 384, Barcelona, 1879–1881.

[137] *Grand dictionnaire universel du XIXe siècle* **5**: 66, Paris, 1865–1884, citing Czyński.

[138] *Vie de Copernic et histoire de la découverte du système du monde*, 62, Paris, 1872 = Flammarion's *L'Astronomie et ses fondateurs: Copernic et la découverte du système du monde*, 65, Paris, 1891. The two later editions (Paris, 1898 and 1916) cited by Baranowski, *Bibliografia kopernikowska*, 94, are, I take it, ghosts.

[139] Ed. 1872: 63; ed. 1891: 66. The translation of Flammarion into Polish by Filip Sulimierski under the title *Życie Mikołaja Kopernika*, Warsaw, 1873, reminds us of the proverbial bread cast upon the waters. Flammarion was translated also into Czech under the title *Kopernik a soustava světová* by Čenčk Ihl (Prague, 1900). The translation of Flammarion into Spanish under the title *Vida de Copérnico*, 57–58, by Mariano Urrabieta (Paris and Mexico City, 1879) produced repercussions. On the one hand, the *Diccionario enciclopedico hispano-americano* **5**: 991 said that in 1503 Copernicus returned "to Kraków, where he became a priest" (Barcelona, 1890); and more recently, in a collective volume entitled *Nicolás Copérnico*, 15, Bogotá, 1943, Belisario Ruiz Wilches stated that Copernicus "received holy orders in Kraków," and Oscar Miró Quesada called him a priest (*Copernico: su vida y su obra*, 83, Lima, 1950). On the other hand, Escalante wrote that Copernicus "never received priestly orders and probably not even the subdiaconate, despite the flat assertion of Camille Flammarion, who declares without any basis that John Konarski, bishop of Kraków, conferred the priesthood on him" (Nicolas Copérnico: su vida, su obra, *Memorias y revista de la academia nacional de ciencias* [Mexico] **55**: 287, 1935–1944).

of 1502, and took holy orders there in 1503." [140] The persistence of this misconception in the twentieth century may be seen in a history of astronomy which disputed the motive adduced by Czyński and Figuier to explain Copernicus' selection of a status which in reality he did not select:

> The leisure time left him by his occupation permitted Copernicus to devote himself to his favorite study, astronomy. It has even been insinuated that he became a priest only for this purpose. But this insinuation is not justified. It is nobler to think that Copernicus, a modest Christian and unambitious scholar, was eager to isolate himself from the world in order to combine his theological meditations with his scientific meditations, and that he was sincerely a priest and sincerely an astronomer. The care which he took to place his masterpiece under the protection of Paul III (who was pope from 1534 to 1549) proves how attached he was to religion in seeking the support of the highest authority which a priest can invoke. It is also an error, in my opinion, to have him take holy orders as late as 1502, and obtain his canonry eight years afterwards.[141]

A commemorative lecture delivered in 1953 continued to speak of Copernicus as a priest,[142] and may serve as our last example to illustrate the extraordinary range and duration of Czyński's baneful influence on French thinking about Copernicus.

IV. OBSTACLES TO THE MYTH'S SPREAD

The claim that Copernicus was a priest spread unchecked until it came to the notice of Franz Hipler (1836–1898), who was himself a Roman Catholic priest. Hipler was also a canon of the Frombork chapter, to which Copernicus had once belonged. As an ardent student of Copernicus' life, Hipler knew that when the Frombork chapter in 1501 granted Copernicus an extension of his educational leave, it explained that it did so "principally because Nicholas promised to study medicine, in order that he might thereafter as a practitioner of the healing art advise our most reverend bishop and also the canons of the chapter." [143] As a professor of theology and head of a seminary for the training of Catholic priests, Hipler commented on the chapter's action as follows:

> At first glance it may seem that the Frombork chapter, by wishing to have in one of its members a practicing physician, perhaps committed a violation of canon law, which, as is well known, never desired or favored the practice of medicine by servants of the church. The regular clergy were strictly forbidden to study medicine at the universities, and those doctors and surgeons whose practice entails burning and cutting are canonically disqualified for ordination as priests because they are deficient in tenderness. Therefore most medieval medical men, particularly the cathedral chapters' canon-doctors or canon-physicians, who often had their own benefices, were actually to have been, not priests, but merely simple clerics. When Copernicus entered the chapter, he must have received the four minor orders [doorman, reader, exorcist, and acolyte] if he did not already have them. Whether he ever took the three major orders [deacon, priest, and bishop] is doubtful, especially since the overwhelming majority of the Frombork canons in the fifteenth and sixteenth centuries are known not to have been ordained as priests. But if Copernicus was not a priest, as a canon he could study and practice the art of medicine, to which he now wanted to devote himself, without even the semblance of an obstacle in his path.[144]

The first obstacle in the path of the propagators of the falsehood that Copernicus was a priest was erected by the priest Hipler. Although he confined himself to expressing a doubt, and uttered no categorical denial, he did underline the incompatibility of the priesthood with the practice of surgery and cautery.

Contributing nothing further to this argument, Leopold Prowe (1821–1887) changed Hipler's doubt to a categorical denial: "When Copernicus entered the chapter, like most of his fellow-canons

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[140] *La grande encyclopédie* **12**: 897, Paris, 1891; the article on Copernicus was written by Léon Sagnet, who cited Czyński at **12**: 899.

[141] Félix Boquet, *Histoire de l'astronomie*, 253, Paris, 1925. Copernicus was called a priest also by Jean Plattard, Le Système de Copernic dans la littérature française au XVIe siècle, *Revue du seizième siècle* **1**: 223, 1913.

[142] Paul Labérenne, Nicolas Copernic, *La Pensée*, no. 50: 29, 1953 (September–October).

[143] Hipler, *Spicilegium copernicanum*, 267. Prowe (**1** (1): 291) omitted a "C" from the date of this document. In 1215 the fourth Lateran Council forbade a "subdeacon, deacon or priest to practice that part of surgery which involves burning or cutting" (see Karl Joseph von Hefele, *Histoire des conciles* **5**: 1348, Paris, 1907–1952). This rule entered the canon law as a section of Chapter IX, Title 50, Book III of the Decretals issued by Pope Gregory IX in 1234 (see *Corpus iuris canonici*, ed. Emil Friedberg, **2**: 660, Leipzig, 1879–1881). In the Code of Canon Law, promulgated by Pope Benedict XV in 1917, Canon 139, Section 2, prohibits clerics "from practicing medicine or surgery without papal permission" (see *Codex iuris canonici*, ed. Pietro Gasparri, 34, New York, 1918).

[144] Hipler, *Nikolaus Kopernikus und Martin Luther*, 27–28, Braunsberg, 1868; printed also in ZE **4**: 501–502, 1867–1869. Hipler's contention that Copernicus was confined to the minor orders by the canonical disqualification for the priesthood of those who practiced surgery and cautery was promptly endorsed by Cesare Cantù, Copernico, *Archivio storico italiano* **13**: 136, 1871 (3d series).

had received only the four minor orders, and did take the major orders afterwards." [145]

Coming from the author of what is still our st substantial biography of the astronomer, owe's outright denial exerted a marked influ- e on subsequent discussion of Copernicus' sup- ed priesthood. Thus Maximilian Curtze (1837– 03), who had supervised the 1873 edition of *Revolutions,* repeated Prowe's thesis that "when pernicus took his place as a canon, he received minor orders, but never the rest." [146] Yet rtze inconsistently slipped back into the pre- olerian position by calling Copernicus a priest ile weirdly arguing that "as a priest Copernicus uld hardly have regularly attended the lec- es" [147] on surgery at the University of Padua.[148] Curtze's inconsistency was underscored by olf Müller (1853–1939), professor of astron- y at the Gregorian University in Rome and di- tor of the private observatory on the Janicu- ı. In a biography of Copernicus published the r before Curtze's, Müller had implicitly re- ted Prowe's denial that Copernicus was a priest repeating Hipler's doubt:

Whether Copernicus ever received the major orders cannot be positively ascertained. To be sure, many writers talk about the priest-astronomer without, however, being able to adduce a trustworthy source for this designation. Apart from the fact that in those days by no means all canons were ordained as priests, in our particular case many reasons seem to indicate that Nicholas was satisfied to receive the minor orders, in order thus to be able better to follow his new (medical) calling and his inclination to study and privacy.[149]

This is the first time, so far as I know, that anybody ever pointed out the complete absence of any documentary foundation for the asserted priesthood of Copernicus. Moreover, Müller rendered a further service to the cause of Copernican scholarship when the translation of his biography from German into Italian [150] gave him an opportunity to make some revisions. Not only did Müller now emphasize Curtze's inconsistency in labeling Copernicus a priest while maintaining that he received only the minor orders, but Müller also named Galileo as one of the writers who talked about the priest-astronomer Copernicus.[151] Thus the two previously separate phases of the Copernicus-priest myth were for the first time brought together by Müller, who differed from other Copernicus specialists in being familiar with the Galileo story too.[152] In addition Müller succeeded in eliciting from Hipler a more outspoken expression of his attitude toward one aspect of the myth. In a letter Müller requested Hipler's opinion about Flammarion's assertion that Copernicus was ordained a priest by John Konarski, bishop of Kraków. "An audacious fabrication," replied Hipler.[153]

[145] Prowe 1 (1): 293; at 1 (1): 158, Prowe scoffed those Polish writers who professed to know that ernicus was ordained a priest at Kraków.

[146] Curtze, Nicolaus Coppernicus, *Himmel und Erde* 203, 1899.

[147] *Himmel und Erde* 11: 207, 1899. In *Sun, stand u still,* New York, 1947, Angus Armitage was simi- y inconsistent: "in Copernicus' time, the Frauenburg upter could barely muster one priest for the service he altar" (p. 95); Copernicus "must have passed for exceptionally favorable specimen of the priesthood" 150; repeated at pp. 78, 121 in *The world of Coper- s,* New York, 1951). For the special twist given he myth of Copernicus' priesthood by dating his sup- ed ordination before (not after) he returned from studies in Italy, see n. 198, below.

[148] Against Curtze's weird argument that a priest uld not attend lectures on surgery, an equally weird ument was proposed: "clergymen were not forbidden study medicine, provided that they did not practice it money"; "Copernicus . . . practiced the healing art, hout a monetary purpose, since he was a clergyman" useppe de Florentiis, Nicola Copernico, *Sapere* 17– 366–367, 1943). An even weirder attempt to preserve legend that Copernicus was a priest cropped up in a ch article commemorating the astronomer's quadri- tennial: "Because he served as a doctor, it has been ught that he never was ordained as a priest. The ctice and profession of medicine were indeed forbidden priests in general. In fact neither a day nor a year ordination is known in Copernicus' case. More accu- ly, the ban recognized many exceptions and dispen- ons, and tradition quite properly sees in Copernicus riest" (S. P. van't Hof, Nicolaus Coppernicus, *De tuur* 63: 49, 1943).

[149] Adolf Müller, *Nikolaus Copernicus, der Altmeister der neuern Astronomie,* 25–26, Freiburg im Breisgau, 1898; printed also in *Stimmen aus Maria-Laach,* Ergänzungsheft 72: 25–26, 1898.

[150] *Niccolò Copernico,* translated by Pietro Mezzetti, Rome, Desclée e Lefebvre, 1902.

[151] *Op. cit.,* 35.

[152] The year before his biography of Copernicus appeared in German, Müller published: Die Sonnenflecke im Zusammenhang mit dem copernicanischen Weltsystem: ein Beitrag zur Galilei-Literatur, *Stimmen aus Maria-Laach* 52: 361–372, 1897, and in the same journal (56: 534–551, 1899): Die Erscheinungen von Ebbe und Flut im Zusammenhang mit dem copernicanischen Weltsystem: ein weiterer Beitrag zur Galilei-Literatur. Müller's *Galileo Galilei und das kopernikanische Weltsystem* and *Der Galilei-Prozess* were both printed at Freiburg im Breisgau in 1909 as well as in *Stimmen aus Maria-Laach,* Ergänzungsheft 101–102, 1909.

[153] Müller, *N. Copernico,* 35. The "audacious fabrication" was amplified by Ignacy Polkowski (1833–1888), who was himself a priest. In his *Żywot Mikołaja Kopernika,* 158, Gniezno, 1873, he applied the principle of

V. DENIAL, DOUBT, AND AFFIRMATION

All in all, then, we have seen three competing attitudes emerge toward the claim that Copernicus was a priest: (*a*) the affirmation, which was started in 1615 by Galileo, who was afterwards ably assisted by the Polish team of Nakwaska, Krzyżanowski and Czyński; (*b*) the doubt, which was initiated in 1868 by Hipler; and (*c*) the denial, which was begun in 1883 by Prowe.

Prowe's denial, embedded in what was at once recognized to be the outstanding biography of Copernicus, had an immediate effect, which can be seen with startling clarity in the case of *Chambers's Encyclopaedia*. This standard reference work had proclaimed, prior to the publication of Prowe, that Copernicus "entered into holy orders";[154] but the first post-Prowean and subsequent editions said that he "never became a priest."[155] In like manner the first post-Prowean revision of the *Encyclopaedia Britannica* agreed that Copernicus "never took orders."[156] "He never took orders," promptly echoed *Everyman's Encyclopaedia*.[157] In Prowe's own country the *Handbuch der Romfrage*, edited by the Nazi philosopher Alfred Rosenberg (1893–1946), declared that Copernicus obtained a canonry "without having received priestly ordination or major orders in general,"[158] and the sixteenth edition of *Der grosse Brockhaus* stated that Copernicus "took only the minor orders."[159]

Not only collective works but also books written by single individuals reflected Prowe's influence. For example, an inadequately informed author remarked that Copernicus "had taken the lesser vows for the priesthood, but he never took the higher ones which would have formally ordained him."[160] The eminent historian of science George Sarton (1884–1956) held that "Copernicus may have been in minor orders but was never ordained a priest."[161] The novelist and journalist Arthur Koestler (1905–) denied that Copernicus "took Holy Orders."[162] A Peruvian professor explained that

> during Copernicus' stay in Italy, through the influence of his uncle the bishop he was elected a canon of Varmia. This has caused some people to suppose, erroneously, that Copernicus was a priest. It seems proved that he was not, and in any case at that time it was not necessary to be a priest in order to be a canon, since the office of canon was entirely different from what it is now, and the functions connected with it were absolutely unlike the present functions.[163]

This difference was set forth more explicitly by an East German professor, who had a realistic understanding of the conditions that prevailed in Copernicus' time:

> Most of the canons, especially those in Varmia, did not take even the minor orders. In practice the canonries were benefices, which assured their possessors an unearned income and gave them a social status like that of the secular nobility. Hence it is not at all surprising that in particular the sons of the wealthy urban families strove to obtain these

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the division of labor to Copernicus' imaginary ordination: at Kraków, "having prepared himself in theology for priestly ordination, he could receive the subdiaconate and diaconate from the hands of James Zaremba of Bydgoszcz, the suffragan of Kraków, formerly the friend of his father, and the final priestly ordination from the hands of John Konarski, bishop of Kraków." As the foundation of his fanciful superstructure, Polkowski cited the crumbling sand provided by Krzyżanowski, Czyński, and Figuier.

154 Edd. London, 1868, 1874, 1884 **3**: 225.

155 Ed. 1888–1892 **3**: 461; ed. 1923–1927 **3**: 458 (distributed outside England under the title *British universities encyclopaedia*). The denial that Copernicus was a priest was omitted from the new article written by the Astronomer Royal Harold Spencer Jones for the 1950 ed. **4**: 110–111 (reprinted in 1955).

156 Eleventh ed. **7**: 100, Cambridge, England, and New York, 1910–1911; the historian of astronomy Agnes Mary Clerke (1842–1907) contributed this article on Copernicus, which was reproduced in the fourteenth ed. (1929) and later reprints.

157 First ed. **4**: 336, New York, 1913–1914; 2d ed. **4**: 377, 1931–1933; 3d ed. **4**: 235, 1949–1950; 4th ed. **3**: 778, 1959.

158 **1**: 809, Munich, 1940.

159 **6**: 554, Wiesbaden, 1955.

160 Ginzburg, *Adventure of science*, 84; see n. 3, above, for Ginzburg's error in calling Copernicus a monk. Ginzburg evidently tried to follow Edward Singleton Holden (1846–1914), director of the Lick Observatory, who said that Copernicus "had already taken the lesser vows; but the higher he never was dedicated" (Copernicus, *Popular Science Monthly* **65**: 111, 1904, reprinted in *Scientific American Supplement* **58**: 24066, 1904). But in that same article Holden called "Copernicus a Catholic priest" (*Pop. Sci. Mo.*, 125; *Sci. Amer. Sup.*, 24082). Holden had previously made this error in the article on Copernicus (omitted from Baranowski's *Bibliografia kopernikańska*), which he contributed to the *Library of the world's best literature*, 4042, New York, 1896–1897 (reprinted as the *Warner library*, New York, 1917) and in his book for children, *Stories of the great astronomers*, 52, New York, 1900 (re-issued, New York and London, 1912).

161 Sarton, *Six wings: men of science in the Renaissance*, 256, Bloomington, 1957.

162 Koestler, *The sleepwalkers*, 128, London, New York, 1959, in agreement with Kesten, *Copernicus and his world*, 109; *Copernicus und seine Welt*, 139, Amsterdam, 1948.

163 Cristóbal de Losada y Puga, *Copérnico*, 21, Lima, 1943; printed also in *Revista de la universidad católica del Perú* **11**: 163–164, 1943.

itions. Furthermore the cathedral chapter as a porate body was the feudal landlord of its domain, administration of which, together with the strug- to maintain or expand their privileges, occupied canons far more than did any spiritual obliga- is.[164]

The astronomer Jean Mascart (1872–1935), ile asserting that "Copernicus was never or- ned a priest," inconsistently declared that after :nding the University of Kraków he "momen- ly abandoned the idea of entering the or- s."[165] "He soon abandoned his early idea of ing holy orders," echoed *Everyman's Encyclo- dia*,[166] implying, however, that the abandon- it was permanent, not momentary. This im- ation was shared by the *Grande enciclopédia tuguesa e brasileira:* "He abandoned the idea ch he had of taking orders."[167] Whether Co- nicus ever thought of becoming a priest, and ther he ever abandoned the idea either mo- itarily or permanently, are questions about ch nothing is known. It is known, however, the first attempt by Copernicus' uncle to ain a canonry for him in 1495 fell short of the red goal, whereas the second attempt in 1497 ceeded.[168] Can these facts have been some- transmogrified into a momentary abandon- t of the idea of entering the priesthood?[169] n any case Prowe's categorical denial that ernicus was a priest won much wider support Hipler's cautious state of indecision which, e saw above in Section IV, was endorsed by ler.[170] The latter's emphasis on the absence idence was repeated by Johann Georg Hagen 47–1930) in the *Catholic Encyclopaedia:* "There is no document to show that Copernicus ever received higher orders."[171] Nevertheless, the director of the Vatican Observatory went on to say that Copernicus' "medical practice, which was only private, would not speak against him being a priest; and the fact that in 1537 King Sigismund of Poland put his name on the list of four candidates for the vacant episcopal see of Ermland, makes it probable that, at least in later life, he had entered the priesthood."[172] This alleged probability was promoted to the rank of a certainty by the *Enciclopedia universal illustrada europeo-americana*: "Although no documents exist which prove that Copernicus received holy orders, there can be no doubt about it, since in 1537 King Sigismund of Poland put his name on the list of candidates for the vacant see of Ermland."[173] But the vacant see of Ermland (or Varmia) was filled in 1537 by a procedure which required the king of Poland to choose his four candidates from the canons of the Frombork cathedral chapter,[174] and "the overwhelming majority of the Frombork canons in the fifteenth and sixteenth centuries are known not to have been ordained as priests."[175] Hence the candidates for the Varmia bishopric need not have been priests, and few were. Therefore, Father Hagen's argument that Copernicus entered the priesthood, at least in later life, is seen to be based on a lack of familiarity with the Varmia diocese in Copernicus' lifetime.

If Hipler's doubt whether Copernicus was a priest gained less support[176] than Prowe's denial, the same cannot be said about the affirmation. For instance, the astronomer Karl Ludwig von

Alfons Kauffeldt, *Nikolaus Kopernikus: der Umdes mittelalterlichen Weltbildes*, 103, Berlin, 1954. *Nouveau Larousse illustré* **3**: 259, Paris, 1900. The *onario enciclopedico hispano-americano* **5**: 991, Barca, 1890, had pretended to know that Copernicus re- d from Kraków "with the intention of entering the s, but momentarily abandoned this project." Mo- ry abandonment is consistent with later entrance the priesthood (the position taken by this encyclo-) but inconsistent with Mascart's denial that Coper- ever became a priest.

First ed. **4**: 336; 2d ed. **4**: 377; 3d ed. **4**: 235; 4th 778.

7: 619, Lisbon and Rio de Janeiro, 1940.

Prowe **1** (1): 172–175; ZE **24**: 454–459, 1930– ZE **25**: 242–243, 1933–1935.

The *Diccionario enciclopedico hispano-americano* **5**: signed to 1495 Copernicus' momentary abandonment plan to enter the priesthood.

The state of uncertainty whether Copernicus ever ed the major orders was transmitted from Müller rl Franz Hoffmann, Nikolaus Coppernikus als Arzt, *krates* **14**: 444, 1943.

[171] **4**: 352, New York, 1908.

[172] *Op. cit.* **4**: 352–353. Father Hagen's mistaken conclusion was repeated by Michael Joseph Ahern (1877–), an American Jesuit and ordained priest, who said that Copernicus "in all probability was ordained to the Catholic priesthood" (*Nicholas Copernicus, a tribute of nations*, 20, New York, 1945, ed. Stephen P. Mizwa).

[173] **15**: 389, Barcelona, *ca.* 1925.

[174] Prowe **1** (2): 35, 323; for a detailed account of the election, see Anton Eichhorn, Geschichte der ermländischen Bischofswahlen, ZE **1**: 323–335, 1858–1860. The bishop chosen in 1512 was only a subdeacon, as Müller pointed out (*N. Copernicus*, 34).

[175] Hipler, cited in the text above at n. 144.

[176] Hipler's hesitant attitude reappeared forty-five years later in a popular history of astronomy by Ottavio Zanotti-Bianco (1852–1932): "Copernicus studied medicine too. This fact makes it doubtful that he ever received the major religious orders, but only the minor. At that time not all the canons were priests, but simple clerics who had received only the minor orders" (*Storia popolare dell' astronomia*, 37–38, Turin, 1913).

Littrow (1811–1877) referred to "Copernicus the priest."[177] The Catholic apologist Paul Schanz (1841–1905) blithely stated that "Copernicus is always designated a priest."[178] The professor of the harmony of science and revealed religion in Princeton University, Charles Woodruff Shields (1825–1904), who was himself an Episcopalian priest, declared that "Copernicus lived as a faithful priest."[179] The physicist and spiritualist Oliver Lodge (1851–1940) wrote that Copernicus "became an ecclesiastic"[180] and "went into orders. On his return home, he took charge of the principal church in his native place."[181] The Royal Astronomer of Ireland Robert Stawell Ball (1840–1913) echoed that "Copernicus took holy orders."[182] He "entered into holy orders," chorused the *New International Encyclopaedia*[183] and the *Larousse du XXe siècle*.[184] Although the sociologist Erich Kahler (1885–) subtitled one of his books "A new approach to history," he adopted no new approach to the myth that Copernicus was a priest, which he simply repeated.[185] So did the cultural anthropologist Alfred Louis Kroeber (1876–).[186] "Copernicus, a Polish priest," participated in *The Pilgrimage of Western Man*[187] as viewed by the historian Stringfellow Barr (1897–). Our last witness to the durability of the Galileo-Krzyżanowski fable is the historian of science Frank Sherwood Taylor (1897–1956), who spoke of "the Polish priest, Copernicus."[188]

VI. THE REJUVENATION OF THE MYTH

Although Prowe's authority as the foremost biographer of Copernicus remained unchallenged, his denial that Copernicus was a priest failed to win universal consent, as the foregoing examples of the contrary opinion have amply demonstrated. The priesthood of Copernicus continued to be asserted for decades despite Hagen's admission that "there is no document to show that Copernicus ever received higher orders."[189] Then precisely such a document (or rather, what was said to be such a document) was found.

It was discovered in Bologna, where Copernicus enrolled as a student at the university in 149[illegible]. His uncle, the bishop of Varmia, had first tried to have him elected a canon of the Frombork cathedral chapter in 1495 without complete success, as we saw above.[190] But in 1497 the second attempt succeeded, while Copernicus was far away in Italy. Rather than make the long trip to Frombork for the purpose of taking possession of his benefice in person, he preferred to remain in Bologna and there empower two of his friends back home to act for him. Accordingly he visited a Bolognese notary, who executed the necessary document. This paper authorized the two friends of Copernicus

in his name and on his behalf, to receive, accept, and opt all the unencumbered lands, possessions, goods both movable and immovable, rights, actions, income and benefits due him from any canonry that was still vacant.[191]

[177] *Kalender für alle Stände*, Beilage I: 19, 1873 (unsigned, but listed as one of Littrow's publications in the obituary by Edmund Weiss, *Almanach der k. Akademie der Wissenschaften*, Vienna **28**: 208, 1878).

[178] Copernicus, *Wetzer und Welte's Kirchen-Lexicon*, 2d ed. **3**: 1081, Freiburg im Breisgau, 1884. The popular novelist Zsolt Harsányi (1887–1943) also called Copernicus a priest in his fictional biography of Galileo, *The star-gazer*, 149, New York, 1939, translated from the Hungarian by Paul Tabor.

[179] *Philosophia ultima* **3**: 160, New York, 1888–1905. Shields likewise pretended to know that Copernicus "died requesting that his epitaph might be the prayer of the penitent thief on the cross, 'Lord, remember me when thou comest in thy Kingdom.'"

[180] This ambiguous epithet (*Pioneers of science*, 4, London and New York, 1893) may have been copied by Lodge from Whewell's *History of the inductive sciences* (**1**: 377, London, 1837; 2d ed. **1**: 397, London, 1847; 3d ed. **1**: 267, New York, 1858; German translation by Joseph Johann Littrow, *Geschichte der inductiven Wissenschaften* **1**: 392, Stuttgart, 1840–1841). Whewell's description of Copernicus as an ecclesiastic may in turn have been modeled after "Geistlicher" in Johann Heinrich Westphal, *Nikolaus Kopernikus*, 40, Constance, 1822.

[181] This mass of misinformation was inserted in the original edition of 1893 at p. 11, and was repeated in eleven reprints of *Pioneers of science* from the corrected edition of 1904 to 1930.

[182] *Great astronomers*, 32, London, 1895. Copernicus was ordained a priest also by Edward J. C. Morton, *Heroes of science: astronomers*, 41, London and New York, 1882.

[183] First ed. **5**: 387, New York, 1902–1904; 2d ed. **6**: 54, 1914–1916.

[184] **2**: 461, Paris, 1929; this article was written by the astronomer Lucien Rudaux (1874–1947), who evidently wished to counteract Jean Mascart's denial in the *Nouveau Larousse illustré* that Copernicus was a priest (see n. 165, above).

[185] *Man the measure*, 679, New York, 1943; reissued 1956.

[186] *Configurations of culture growth*, 173, Berkeley and Los Angeles, 1944.

[187] P. 80, New York, 1949.

[188] *An illustrated history of science*, 30, London and New York, 1955; see also *Tablet* (London) **181**: 245, May 22, 1943.

[189] See the text at n. 171, above.

[190] See the text at n. 168, above.

[191] Lino Sighinolfi, Domenico Maria Novara e Niccolò Copernico, *Studi e memorie per la storia dell' universi-*

: two friends were to represent

holas Copernicus, son of the late Nicholas, canon Frombork, student at Bologna, candidate for a ree in canon law, *presbiter constitutus* in the ence of me, the notary, and of the undersigned nesses, who were specially called and summoned this purpose.[192]

: first of the two italicized words was read the historian Lino Sighinolfi (1876–1956).) unearthed the document, as *presbiter*, a Latin 'd for "priest."

although Sighinolfi cited Prowe by title,[193] he evidently unaware of Prowe's denial that ernicus was a priest. Hence, without any ussion whatever and without even calling ation to what he was doing, Sighinolfi calmly lied to Copernicus both the Latin term *biter* and its Italian equivalent *prete*, apparly without realizing that the document he had overed could be used to silence Prowe's denial terminate Hipler's doubt.

ighinolfi's document was so used, however, - a decade later when it came to the notice of holar who had something more than the barest naintance with the literature concerning Copers. Eugen Brachvogel (1882–1942), who was self a Catholic priest, knew that "the ordina- of Copernicus as a priest was not attested any source."[194] In fact, shortly before chvogel made this statement, a new source just been published: in witnessing a power ttorney for his uncle the bishop, Copernicus described as a simple cleric (and not as a st) on February 22, 1496.[195] But the Bolo- e notarial document, in which Brachvogel uncally assumed that Sighinolfi had read the d *presbiter* correctly, was originally dated September 10, 1497.[196] Therefore, Brachvogel concluded, Copernicus was ordained a priest after February 22, 1496, and before September 10, 1497.

At some undetermined time within this period of a little over a year and a half, Brachvogel reasoned, the ordination of Copernicus took place, "perhaps even before his departure for Italy, before the autumn of 1496"; and he was ordained "by his uncle, Bishop Lucas Watzenrode."[197] In rejuvenating the myth of Copernicus' priesthood, then, Brachvogel advanced the date of the ordination some six years from 1502 to 1496; transferred the site of the ceremony from Kraków to Varmia; and cast Bishop Lucas Watzenrode[198] in the role of presiding prelate instead of the more familiar figures of Bishop John Konarski and Suffragan James Zaremba (who never existed).

After Brachvogel's death in 1942, the foremost German student of Copernicus' life was Hans

ologna **5**: 232–233, 1920. This date was mistakenly ted as "1922" by Hans Schmauch, Neue Funde zum nslauf des Copernicus, ZE **28**: 57, 1943.

Sighinolfi, 232.

Op. cit., 216.

Brachvogel, Zur Koppernikusforschung: Des Kopkus Priesterweihe, ZE **25**: 242, 1933–1935. Brach- went too far, however, in saying that Copernicus' thood was not attested "by any source or tradition" ch keine Quelle oder Ueberlieferung). In adding ast two words Brachvogel disregarded the tradition ted by Galileo and fostered by Czyński and a host her authors. Can that tradition have remained unn to so industrious a student of Copernicus' biogr as Brachvogel? For a list of his writings about rnicus, see ZE **28**: 29–31, 1943.

Hans Schmauch, Zur Koppernikusforschung: Der itt des Nikolaus Koppernikus ins ermländische capitel, ZE **24**: 458–459, 1930–1932.

[196] Intending to alter the document's original date ("September 10") to "October 20," the notary changed the name of the month, but forgot to cancel *decimo* ("tenth") when he inserted *vigesimo* ("twentieth"). Over this already jumbled Latin text Sighinolfi's heading (p. 232) reads "23 ottobre 1497." Sighinolfi's incorrect heading misled Giulio Righini, who was an attorney, into dating the notarial document October 23, 1497, in his article, Copernico "Doctor Ferrariensis" e "Magister" a Bologna, R. Deputazione di storia patria per l'Emilia e la Romagna, sezione di Ferrara, *Atti e memorie* 1: 151, 159, 1942. Righini had previously made the same mistake in his book *La Laurea di Copernico allo Studio di Ferrara*, 238 (misnumbered 328), Ferrara, 1932, although at p. 151 he had given the document's correct (or rather, corrected) date as October 20, 1497. At p. 196 in his book Righini misdated Galileo's *Letter to the Grand Duchess* in 1614 (the reason for this error is explained by Edward Rosen, *Isis* **49**: 319, 1958). It was through Righini's book that Brachvogel learned about Sighinolfi's document (ZE **25**: 244, 1933–1935).

[197] ZE **25**: 243, 554, 1933–1935; *Hochland* **38**: 56, 1940–1941.

[198] Although Brachvogel carefully refrained from saying so, he was not the first script-writer whose imagination dreamed up the scene of Copernicus' uncle presiding at his ordination. J. M. Watterich, a professor in Braunsberg, did the same in his *De Lucae Watzelrodc episcopi varmiensis in Nicolaum Copernicum meritis*, 15, Königsberg, 1856. But Watterich made the fatal mistake of dating Copernicus' ordination by his uncle at the beginning of the year 1497, when the uncle was in his diocese and Copernicus was studying at Bologna. This chronological hole was pricked in Watterich's pretty balloon by Polkowski, *Żywot Mikołaja Kopernika*, 159. But Polkowski, who was himself a priest, contradicted those who denied that Copernicus was a priest. In the absence of evidence and with the usual non-probative arguments Polkowski insisted that Copernicus was ordained between 1504 and 1507 by Konarski and Zaremba.

Schmauch (1887–). Referring to the Bolognese notarial document, Schmauch wrote:

> If it explicitly designates Copernicus as *presbiter constitutus*, then no objection can be raised to the conclusion that he was a priest, and therefore not merely a simple cleric, as had previously been generally assumed. When did Copernicus receive his priestly ordination, it may be asked. From a power of attorney found in the government archives at Königsberg we know that he was still described as a simple cleric on February 22, 1496. Hence his ordination as a priest must be placed between this fixed day and the day on which the first of the documents published by Sighinolfi was drawn up (September 10, changed to October 20, 1497). His uncle, Bishop Watzenrode, would have ordained him, it may be supposed, even before his departure for Italy.[199]

In an article contributed to a collective volume, whose foreword was written by a Nazi Gauleiter, these hesitations vanished from view as Schmauch announced with complete finality:

> Formerly the general opinion was that Copernicus took only the so-called minor orders, which fully sufficed for a canon in those days. After more recent documentary discoveries, however, now there can no longer be any doubt about the fact that our astronomer was a priest. He must have been ordained between February 1496 and October 1497, when for the first time he is called a priest in any document, by the hand of his uncle, the bishop.[200]

Schmauch was by no means the only German who repeated the Copernicus-priesthood myth as rejuvenated by Brachvogel. Thus Fritz Kubach (born 1912), who was selected by the Nazis to edit the *Nikolaus Kopernikus Gesamtausgabe*,[201] accepted Sighinolfi's reading of *presbiter* as the basis for maintaining that Copernicus "must have been ordained as a priest."[202] A Nazi district leader declared that "probably in the autumn of 1496 Bishop Watzenrode ordained his nephew a priest."[203] A Nazi propagandist dated Copernicus' ordination as a priest "in 1496 or 1497."[204] Another belligerent Nazi deviated in one detail from Brachvogel's form of the myth by insisting that Copernicus was ordained as a priest after his return from Italy.[205] "Even before I began my trip, I was ordained as a priest by my uncle," said Copernicus as a character in the novel by Fritz Meichner (1895–), who thereby clung with greater loyalty to the Brachvogel myth.[206] It was accepted also by the regional historian Leonid Arbusow, Jr. (1882–1951),[207] Ernst Zinner (1886–), the outstanding German historian of astronomy and former director of the Remeis Observatory in Bamberg, referred to the Bolognese document of October 20, 1947, in which Copernicus "is described as a canon and priest. Consequently he had already been ordained as a priest. Presumably this happened in Varmia."[208] Zinner was followed by a co-editor of the nautical journal of the German naval observatory.[209] One of the writers now striving to strengthen West German longing for territorial expansion described Copernicus as a priest.[210] An East German professor of astronomy was equally deceived.[211]

Brachvogel's harmful influence was of course not restricted to German authors. Jeremi Wasiutyński (1907–), for instance, a pro-German Pole (or rather, former Pole and present Norwegian), not only repeated Brachvogel's myth but even extended it by saying: "Probably at that

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[199] Schmauch, Nikolaus Kopernikus in Italien, *Die Mittelstelle* **2** (19): 30–31, 1942–1943. Schmauch had previously presented this view in his article, Nikolaus Copernicus—ein Deutscher, *Jomsburg* **1**: 174, 1937; he repeated the passage verbatim in the volume which he edited jointly with Johannes Papritz (1898–), *Kopernikus-Forschungen*, 12, Leipzig, 1943.

[200] Schmauch, Leben und Wirken des Nikolaus Kopernikus, in *Nikolaus Kopernikus, Persönlichkeit und Werk*, 23, Danzig, 1943, ed. August Georgens.

[201] 1, Munich and Berlin, 1944, is a photocopy of Copernicus' autograph manuscript of the *Revolutions;* **2**, Munich and Berlin, 1949, the Latin text of the *Revolutions*, was edited by Franz Zeller and Karl Zeller.

[202] Kubach, Nikolaus Kopernikus, *Die Burg* **2**(2): 10, 1941. Kubach's comprehension of Sighinolfi's document was defective in another respect: according to Kubach, Copernicus appointed only one agent; but according to the document itself, as we saw above in the text at n. 191, Copernicus was prudent enough to appoint two agents.

[203] Thomas Gengler, *Nikolaus Kopernikus*, 12, Göttingen, 1944.

[204] Eugen Oskar Kossmann (using the pseudonym "Dr. K. Müller"), *Nikolaus Coppernicus*, 5, Berlin, 1938.

[205] Franz Strauss, Nikolaus Kopernikus ein Deutscher und Schöpfer eines neuen Lehrgebäudes in der Astronomie, *Nationalsozialistisches Bildungswesen* **7**: 201, 1942.

[206] Meichner, *In der Mitte steht die Sonne*, 44, Munich, 1943.

[207] Arbusow, Livländische Beziehungen von Nikolaus Koppernicks Frauenburger Zeitgenossen, *Quellen und Forschungen zur baltischen Geschichte* **5**: 13, 1944.

[208] Zinner, *Entstehung und Ausbreitung der copernicanischen Lehre*, 158, Erlangen, 1943.

[209] Ernst Römer, Nikolaus Kopernikus, *Der Seewart* **12**: 64, 1943.

[210] Karlheinz Gehrmann, Der Beweger der Erde, p. 203 in *Heimat im Herzen, wir von der Weichsel und Warthe*, ed. Erhard Wittek, Salzburg, 1950.

[211] Albrecht Kahrstedt, Kopernikus als Mensch und Wissenschaftler, *Wissenschaftliche Annalen* **3**: 312, 1949

time [1496] Copernicus received from the hands of his uncle the lower priestly orders, but soon afterwards also the higher." [212] William Lindsay Young, President of Park College, in a commencement address referred to "the Reverend Mr. Copernicus" as "a Catholic priest." [213] An article in the *Carnegie Magazine* declared that Copernicus "entered the priesthood." [214] Miss Evelyn Marian Mance, a member of the British Astronomical Association, wrote that Copernicus "became a priest." [215] While the Dutch astronomer Johan W. Stein (1871–1951) was the director of the Vatican Observatory, he introduced Brachvogel's myth to his Italian colleagues,[216] and later translated it into French.[217] The Flemish cleric Maurice Daisomont (1887–) referred to the Bolognese notarial document of October 20, 1497, as proof that Copernicus was a priest.[218] In a magazine directed and administered by Vatican City a contributor maintained that until the discovery of the Bolognese notarial document the priesthood of Copernicus was a doubtful question,

which was settled by the aforesaid document. The notary, Girolamo Belvisi, was incapable of committing such an error, especially because so important a document was involved. It should be noted also that Nicholas' brother Andrew, who was not a priest, is called a "cleric of Chelmno". . . . Therefore Nicholas Copernicus was already a priest in 1497. This is a small contribution to his biography which has heretofore escaped the students of the subject.[219]

Finally, a book intended to show that the Roman Catholic Church was never hostile to science listed Copernicus as a secular priest.[220]

[212] Wasiutyński, *Kopernik, twórca nowego nieba*, 79, Warsaw, 1938; *cf.* p. 113.

[213] W. L. Young, The greatness of Copernicus, *Quarterly Review of Higher Education among Negroes* **11** (3): 26–27, 1943.

[214] *Carnegie Magazine* **17**: 11, 1943–1944. This unsigned article on "The founder of modern astronomy" may have been written by the editor, Samuel Harden Church, before his death on October 12, 1943.

[215] E. M. Mance, Some centenaries for 1943, *Jour. British Astron. Assoc.* **53**: 65, 1942–1943.

[216] Stein, Copernico era sacerdote? *Memorie della società astronomica italiana* **17**: 3, 1945.

[217] Stein, Copernic était-il prêtre?, Specola astronomica vaticana, *Miscellanea astronomica* **3**: 88–89 (no. 103), 1950.

[218] M. Daisomont, *Copernicus: astronomische Sprokkelingen*, 5, Bruges, 1943; *Le clergé catholique devant l'astronomie*, 14–15; *Ciel et terre* **70**: 433, 1954.

[219] Stanisław Janikowski, Appunti su Niccolò Copernico, *Ecclesia* **12**: 612, 1953. Janikowski misdated Sighinolfi's document "October 27, 1497."

[220] Luigi Maria Torcoletti, *Il processo di Galileo; clero ed astronomia*, 46, 368, Monza, 1956.

Sighinolfi's misreading of *presbiter* was adopted so wholeheartedly by Brachvogel that he regarded it as the documented destruction of Prowe's denial that Copernicus was a priest. He likewise felt that it was the final answer to Hipler's doubt. Three arguments which tended to support Prowe's denial and Hipler's doubt were summarized by Brachvogel as follows: (*a*) Copernicus practiced medicine; (*b*) in those days it was customary for the majority of the Frombork canons not to be ordained as priests; (*c*) a prelacy bestowed mostly on ordained priests was not granted to Copernicus. Making absolutely no effort to refute these three arguments, Brachvogel simply dismissed them,[221] thereby providing us with an excellent example of how history should not be written.

VII. THE BASIS OF THE REJUVENATED MYTH

Other examples of how history should not be written are given to us with lavish generosity by Sighinolfi. He says about Copernicus, for instance, that

> Jerome Cardan and Giordano Bruno knew him and studied him with different aims, but found insurmountable obstacles in the rigid norms of the Council of Trent; perplexed and unconvinced, they remained silent.[222]

But if they remained silent, how did Sighinolfi discover that they were "perplexed and unconvinced"? As a matter of fact neither Cardan nor Bruno remained silent about Copernicus. Each of them expressed his opinion of the astronomer in print. Nor was either of them perplexed. On the one hand Cardan (1501–1576) condemned Copernicanism forthwith, not because of any supposed insurmountable obstacles in the rigid norms of the Council of Trent, but because "it is idle chatter to say that the earth revolves so rapidly without our being aware of it, as Copernicus maintains." [223] On the other hand Bruno

[221] ZE **25**: 243, 1933–1935.

[222] Sighinolfi, 229.

[223] Cardan, *De rerum varietate*, bk. 2, ch. 11 (Basel, 1557: folio ed., p. 55; 8° ed., p. 102; 2d ed., Avignon, 1558, p. 77); at p. 69 in the German translation by Heinrich Pantaleon, *Offenbarung der Natur*, Basel, 1559. In an earlier reference to Copernicus, Cardan was perplexed, not by the question whether Copernicus was right or wrong, but by the entirely different question whether Copernicus followed Archimedes' opinion in the *Sand-Reckoner* to the letter. However, Cardan misunderstood the opinion of both Archimedes and Copernicus. To the latter Cardan erroneously attributed the views that the earth was in the center of the universe, and that the lunar spheres rotated with the sublunar elements (*De*

(1548–1600) was a most forthright admirer of Copernicus, whom he eulogized as

> a serious, subtle, careful, and mature mind; a man who is not inferior to any astronomer who lived before him; . . . in native intelligence he was much superior to Ptolemy, Hipparchus, Eudoxus, and all the others who followed in their footsteps. . . . Who can fully praise the greatness of his intellect?[224]

In an oration delivered at the University of Wittenberg on March 8, 1588, Bruno asked his audience:

> What do you think of Copernicus, not so much as a mathematician but (and this is extraordinary) incidentally as a physicist? He is found to have understood more in two chapters than Aristotle and all the Peripatetics in their entire study of nature.[225]

Sighinolfi's characterization of Bruno as "unconvinced" by Copernicus is so ludicrous that it could not even convince Sighinolfi himself:

> For almost a century the neo-Pythagorean and humanistic idea, scientifically developed by Copernicus, had been neglected and relegated to the field of abstraction and hypothesis, without being able to find any followers except among his faithful disciples beyond the Rhine. Only a few liberal minds in Italy had considered it and appraised it as more than a promising general hypothesis, legitimately descended from the philosophical thought of our humanism and destined for the greatest triumphs of civilization; so it was regarded by Celio Calcagnini, by Jerome Cardan,[226] by Giordano Bruno, condemned by the Inquisition to the silence of the grave.[227]

If Bruno regarded Copernicanism as "destined for the greatest triumphs of civilization," how could he have been "perplexed and unconvinced"?

Sighinolfi's self-contradictory mistakes concerning Cardan and Bruno shrink into insignificance, however, alongside his vicious lies about Galileo. Managing somehow to keep a straight face, Sighinolfi had the audacity to declare that

> Galileo was at fault because, in violation of the prohibition of the Sacred Congregation of the Holy Office, he had continued to profess and teach the condemned theory [Copernicanism] and had composed a book about it, extorting the permission to print and publish.[228]

How the permission to print and publish the *Dialogue* could have been "extorted" from the Master of the Sacred Apostolic Palace in Rome, and from the Vicar General and Inquisitor General of Florence, Sighinolfi of course did not deign to tell us.[229]

Among the witnesses who were present when Copernicus designated the proxies to take possession of his canonry for him, one was said by the notary to come from the *Vurmiensis diocesi* (diocese of Varmia), and another from the *diocesis vuladislaviensis* (diocese of Włocławek).[230] These two dioceses, whose seats are separated by about a hundred miles, were said by Sighinolfi to be the same.[231]

Sighinolfi discovered not only the document of October 20, 1497, in which Copernicus designated his proxies, but also a document executed on June 18, 1499, by the same Bolognese notary Girolamo Belvisi, in the presence of Copernicus as a witness. In this latter document the notary called Copernicus *magister*,[232] an appellation which he did not apply to Copernicus in the earlier document. Sighinolfi noticed the presence of the word *magister* in 1499 and its absence in 1497 as well as in 1496, when Copernicus was enrolled

subtilitate, bk. 17; inserted in the 2d ed., Basel, 1554, p. 454, and not present in the first ed., Nuremberg, 1550, and its reprints).

224 Bruno, *La cena de le ceneri*, 5–6, London, 1584.

225 Bruno, *Opera latine conscripta* **1** (1): 17, Naples and Florence, 1879–1891.

226 Here (p. 231) Sighinolfi lists Cardan among the supporters of Copernicus; previously (p. 229) Sighinolfi described Cardan as perplexed, unconvinced, and silent about Copernicus. Actually Cardan opposed Copernicus (see the text at n. 223, above) in an unperplexed, convinced, and outspoken manner.

227 Sighinolfi, 231.

228 *Op. cit.*, 230.

229 For an account of the "extortion," which lasted a year and a half from start to finish, see NE **7**: 4–8. Giorgio de Santillana, *The crime of Galileo*, 183–18 Chicago, 1955 (218–222 in the French translation by Adriana Salem, *Le procès de Galilée*, Paris, Club du meilleur livre, 1955).

230 Sighinolfi, 233.

231 *Op. cit.*, 216. On May 26, 1918, Sighinolfi delivered a lecture entitled "Da Copernico a Galileo," a brief summary of which was published in *Atti e memorie della r. deputazione di storia patria per le provincie di Romagna* **8**: 297–298, 1917–1918. The page reference to this summary in Giuseppe Boffito, *Bibliografia galileiana 1896–1940*, 96, Rome, 1943, was incorrectly given as "pp. 298 e sgg." Baranowski, *Bibliografia kopernikowska*, 227, made matters worse by miscopying Boffito's incorrect reference as "s. 198 i n." The lecture itself was never printed by Sighinolfi, who did not include it when he listed his publications from 1915 to 1925 in the *Annuario della r. università di Bologna per l'anno accademico 1925–1926*, 133–134. According to the summary, however, Sighinolfi stated that Copernicus sent his proxy document to "Kolm." But Kulm (Chełmno) was a separate diocese from Varmia. For the simultaneous creation of the two dioceses of Varmia and Chełmno, see Prowe **1** (1): 179–180.

232 Sighinolfi, 234.

as a student in Bologna.[233] "Consequently," reasoned Sighinolfi, "the hypothesis presents itself that between 1497 and 1499 Copernicus found the time, or was forced, to earn that degree."[234] "We may think that his degree of Master of Arts . . . must have been earned at the University of Bologna."[235]

Sighinolfi's interpretation of *magister* in the notarial document of June 18, 1499, was uncritically accepted by both Brachvogel[236] and Schmauch.[237] But the attorney Giulio Righini proposed a far more plausible explanation of *magister*:

Sighinolfi immediately thought of a "Master of Arts," that is, a graduate of that group of disciplines which then included the humanities and the sciences, and—among the sciences—mathematics and astronomy. That would entail a Bolognese diploma: a second academic degree earned by Copernicus in Italy.

This interpretation seems too hasty. Indeed, after its occurrence in the Belvisi document, the title *magister* no longer appears alongside Copernicus' name in the immediately succeeding period—and not even in the wording of his Ferrarese doctoral degree, which mentions the various titles of the illustrious student. Therefore it is logical to believe that the appellation was not the official and permanent designation of an academic degree, but indicated a function which was merely temporary and contingent in character.

Magister is simply a master, a teacher, one who imparts instruction: an office which, in Copernicus' time, could be assigned to a simple student entrusted with holding a *lectureship*, that is, developing a course of lessons.[238]

While Copernicus was studying canon law at Bologna, he was already so expert in the mathematical and astronomical sciences, which he had learned at Kraków and pursued with boundless zeal at Bologna under Domenico Maria Novara, that he was able—probably stimulated by financial need—to assume the duty of teaching science at the University of Bologna, as he did later at the University of Rome. The "master" of Bologna is the counterpart of the "professor of mathematics" who holds forth at Rome in 1500 before a crowd of students and a throng of great scientists and specialists in the subject. This is also the answer to the question of various scholars who have not succeeded in explaining to themselves how Copernicus could be a professor at Rome without a doctoral degree or license to teach.[239] He was a "master"—that is, simply a person assigned to teach, seen in the exercise of his occasional functions as an instructor.[240]

Magister in the document of June 18, 1499, was interpreted by Sighinolfi to mean "a holder of the Master of Arts degree."[241] Had Copernicus actually taken that degree, he would have received a lifelong honor: once an M.A., always an M.A. But the term *magister* is never again applied to Copernicus in any later document. Its disappearance, which is utterly irreconcilable with Sighinolfi's interpretation, is explained by Righini's equating of *magister* with "teacher"; for

[233] *Acta nationis germanicae universitatis bononiensis*, edd. Ernst Friedlander and Carlo Malagola, 248, Berlin, 1887; for the corresponding entry in the *Matricula*, see Malagola, *Della vita e delle opere di Antonio Urceo detto Codro*, 313–314, 564, Bologna, 1878.

[234] Sighinolfi, 222.

[235] *Op. cit.*, 224.

[236] ZE **25**: 244, 1933–1935.

[237] *Jomsburg* **1**: 175, 1937. Schmauch in turn was followed by Hans Joachim Schoenborn, Coppernicus der Deutsche, 20, in *Heroen des Geistes im deutschen Osten*, Königsberg, 1939.

[238] For these teachers possessing a status below that of professor, see Albano Sorbelli and Luigi Simeoni, *Storia della università di Bologna* **1**: 175–176, Bologna, 1944–1947.

[239] Allan Kosko, La prétendue chaire d'astronomie de Copernic à la Sapienza de Rome, in Wędkiewicz, *Études coperniciennes* **1**: 283–286; Ryszard Gansiniec, Rzymska profesura Kopernika, *Kwartalnik historii nauki i techniki* **2**: 471–484, 1957 (with a summary in English at pp. 482–484).

[240] Righini, 1942: 152–154. At p. 152 Righini mistakenly supposed it to be probable that Copernicus received a doctoral degree in medicine from the University of Padua; and he dated Copernicus' supposed completion of the Paduan medical course in 1504, when Copernicus was no longer in Italy (see n. 27, above). Righini also misnumbered the document of June 18, 1499, as no. 681 (instead of no. 68) in Belvisi's filza 9, formerly preserved in Bologna's Archivio Notarile (now merged with the same city's Archivio di Stato).

[241] Again it was Brachvogel, as we saw in n. 236 above, who uncritically accepted Sighinolfi's misinterpretation and was primarily responsible for its spread in the German-speaking area. After it was repeated by Brachvogel in the popular magazine *Hochland* **38**: 56, 1940–1941, it was transmitted to the medical profession in *Hippokrates* **14**: 444, 1943, by Karl Franz Hoffmann; to astronomers in *Veröffentlichungen der Universitäts-Sternwarte zu Königsberg* **12**: 3, 1944 by Erich Przybyllok; to geographers in *Geographischer Anzeiger* **45**: 83, 1944 by Franz Strauss; to regional historians in *Quellen und Forschungen zur baltischen Geschichte* **5**: 5, 1944 by Leonid Arbusow, Jr.; and to the academic world in general in *Göttinger Universitäts-Reden* **14**: 13, 1944, by Thomas Gengler. Albrecht Kahrstedt not only repeated the Sighinolfi-Brachvogel misinterpretation, but he also transferred from Bologna to Padua the honor of conferring on Copernicus the M.A. degree, which the astronomer never received (*Wissenschaftliche Annalen* **3**: 312, 1954). The Sighinolfi-Brachvogel misinterpretation was introduced into the English-speaking world by Armitage: "It has now been established that Copernicus took his M.A. degree at Bologna" (*Copernicus, the founder of modern astronomy*, 50, New York and London, 1957).

when Copernicus ceased to be a teacher, the label *magister* was no longer applicable to him.

Exactly the same line of argument may be directed against Sighinolfi's reading of *presbiter* in the document of October 20, 1497. If Copernicus was indeed a priest at that time, why was he never again referred to as a priest throughout the remaining forty-five years of his life? Why did nobody speak of him as a priest for more than seven decades after his death? Why was he first called a priest in 1615?

Yet between the two cases of *magister* and *presbiter* there is all the difference in the world. Copernicus was really called *magister* in the document of June 18, 1499. But was he described as *presbiter* in the document of October 20, 1497?

That document was drawn up, as we have already seen,[242] on behalf of

> Nicholas Copernicus, son of the late Nicholas, canon of Frombork, student at Bologna, candidate for a degree in canon law, *presbiter constitutus* in the presence of me, the notary, and of the undersigned witnesses, who were specially called and summoned for this purpose.

Some twenty months later the same notary drafted the document of June 18, 1499, on behalf of someone else in almost exactly the same terms. I say "almost exactly," because now the word *constitutus* stands before, instead of after, the individual's name and titles. The word immediately following the name and titles was in this instance correctly read by Sighinolfi as *personaliter*. Had he compared the two documents with each other, he would have seen that in the document of October 20, 1497, the notary did not call Copernicus a priest (*presbiter*).[243] On the contrary, he merely stated that Copernicus personally appeared (*personaliter constitutus*) "in the presence of me, the notary, and of the undersigned witnesses, who were specially called and summoned for this purpose." In so doing, the notary employed a traditional legal formula,[244] with which it is difficult to believe that Sighinolfi was unfamiliar.

Of the innumerable examples of this formula, I shall cite only two. On Tuesday, January 10, 1503, a notary in Padua declared that Copernicus "personally appeared (*personaliter constitutus*) in the presence of me, a public notary, and of the undersigned witnesses, who were specially called and summoned for this purpose."[245] On June 22, 1633, Galileo, "appearing personally (*constitute personalmente*) in court, and kneeling before you, most eminent and most reverend cardinals, inquisitors-general in all Christendom against heretical depravity," admitted that the Holy Office had adjudged him "vehemently suspect of heresy, that is, of having held and believed that . . . the earth . . . moves."[246]

Hence we see that Sighinolfi's labeling of Copernicus as a priest had absolutely no foundation in fact. Wittingly or unwittingly, he misread *personaliter* as *presbiter*. The rejuvenated myth that Copernicus was a priest turns out to have been based on nothing more substantial than a paleographical misreading.

VIII. THE RECENTLY DISCOVERED EVIDENCE

As we just saw near the end of Section VII, a recently discovered document disclosed Copernicus' visit to a Paduan notary early in 1503. On that occasion, writing out with his own hand a preliminary draft of what he wanted the notary to attest, Copernicus described himself as a "canon of Varmia and scholaster of the church of the Holy Cross in Wrocław."[247] In his self-description, then, he spoke of himself as holding two ecclesiastical offices: canon and scholaster.[248] But he said nothing about his being a priest, nothing about having been ordained "about 1495"[249] by his uncle the bishop of Varmia, or in 1502 by the bishop or suffragan of Kraków

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[242] In the text at n. 192, above.

[243] But in the document of June 18, 1499, the notary did call another person a priest. For *presbitero* the notary wrote ptro, and psolr or psonalr for *personaliter; cf.* Adriano Cappelli, *Lexicon abbreviaturarum, 293,* 5th ed., Milan, 1954.

[244] For five instances of the use of this formula in Bologna during the period extending from 1380 to 1529, see *Acta nationis germanicae universitatis bononiensis, 395, 396, 399,* 404, 408.

[245] Erice Rigoni, Un autografo di Niccolò Copern[icano] *Archivio veneto* **48-49**: 149, 1951.

[246] NE **19**: 406. Galileo's abjuration of Copernicanism was spoken in Italian; on two earlier occasions in 1633 (April 30 and June 21; NE **19**: 342, 361) the Latin documents used the formula *constitutus personaliter*.

[247] Rigoni, 149. Copernicus' draft was reproduced photographically by Schmauch, *Archiv für schlesische Kirchengeschichte* **12**: opposite p. 154, 1955. Copernicus' reference to himself as "scholaster of the church of the Holy Cross in Wrocław" renders obsolete Prowe's remark that Copernicus was so described "neither by himself nor by anybody else" (**1** (1): 315).

[248] These are the only two ecclesiastical offices enumerated in Copernicus' doctoral diploma of May 31, 1503.

[249] This was the date given by the writer who concealed his identity behind the initials A. D. M. in *L'Osservatore romano* **80** (162): 3, July 15–16, 1940.

We know a little about the circumstances under which Copernicus became a canon of Varmia in 1497, and in the new Paduan document of January 10, 1503, Copernicus stated that the Wrocław "scholastry had been conferred on me recently." [250] Hence he received it shortly before his thirtieth birthday; and he held it about thirty-five years,[251] resigning before February 4, 1538.[252] Four years later he addressed a petition to the Pope, requesting the appointment of his nephew, 'a cleric or scholar in the Włocławek or some other diocese, who is twelve years old or thereabouts," as his "permanent and irrevocable coadjutor" [253] in his Frombork canonry.[254] Less than a year after his petition was granted, Copernicus died, having held his canonry about forty-five years, a decade longer than his scholastry. We have legal and official documents concerning both the inception and the termination of Copernicus' canonry as well as of his scholastry. But concerning his alleged priesthood we have no such evidence.

On the other hand, a recently discovered document provides decisive confirmation that Copernicus was not a priest. After the death of his brother Andrew, who also was a Frombork canon, the customary contest ensued "over the canonry and prebend of the Varmia church which the late Andrew Copernicus possessed while he was alive." [255] On March 30, 1519, one of the rival claimants named a group of thirteen men to act as his proxies. Of the thirteen, one was described as a priest; four as priests and clerics; three as vicars; and the remaining five as canons. One of these five canons was Nicholas Copernicus. The same document calls five other men in the group priests, and does not call Copernicus a priest. Has the argument from silence ever been more eloquent?

A retrospective glance over the past three and a half centuries reveals that Copernicus has been called a priest independently three times: once as an unsupported assertion, once as a revery of a romantic lady novelist, and once as a paleographic misreading. There is no historical foundation whatever for the claim that Copernicus was a priest.[256]

[250] Rigoni, 149: "scolastrie mihi nuper collatae."

[251] A book published in 1559 described Copernicus as "the man from Wrocław." Prowe's condemnation of this description as having "surely arisen from an error, mistaking Varmia" for Wrocław (1 (2): 271) is now seen to have been itself an error.

[252] On that day King Ferdinand of Bohemia appointed "Doctor Joannes Benedictus [Solfa], physician to the King of Poland and canon of Głogow, to the scholastry at the Holy Cross in Wrocław, after the resignation of Nicholas Copernicus"; see *Diplomataria et scriptores historiae germanicae medii aevi* **2**: 27, edd. Christian Schöttgen, Georg Christoph Kreysig, and Heinrich Gottlieb Francke, Altenburg, 1753–1760.

[253] Angelo Mercati, Una supplica di N. Copernico a Papa Paolo III, *Atti della pontificia accademia delle scienze nuovi lincei* **85**: 246, 1931–1932.

[254] Schmauch, Die Gebrüder Coppernicus bestimmen ihre Nachfolger, ZE **27**: 265–273, 1939–1942.

[255] Guido Horn-d'Arturo, Atti notarili del sec. XVI contenenti il nome di Copernico rinvenuti nell' Archivio storico capitolino, *Coelum* **19**: 41, 1951.

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[256] As we saw in n. 200 above, Schmauch formerly adhered to the mistaken claim of Sighinolfi and Brachvogel that Copernicus was a priest. More recently Schmauch admitted that "there is no definite proof that Copernicus ever took holy orders" (*Nikolaus Kopernikus*, 15, Kitzingen, 1953; at p. 22 in the English translation by Helen Taubert, Goettingen Research Committee, Publication **95**, Goettingen, 1954). Perhaps Schmauch recognized the Sighinolfi-Brachvogel blunder when he saw the words *personaliter* (written psonaliter) *constitutus* in a document executed by the notary Girolamo Belvisi in Bologna on August 22, 1497 (ZE **28**: 75, 1943). For expert help in reading the Belvisi documents, I wish to thank Dr. Gianfranco Orlandelli, Assistant Director of the Archivio di Stato in Bologna.

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AN EARLY FLORENTINE POLITICAL THEORIST: FRA REMIGIO DE' GIROLAMI*

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Fra Remigio de' Girolami (d. 1319), Dominican lector at S. Maria Novella, *Magister* in sacred theology, renowned preacher, prolific writer of philosophical treatises, and influential citizen during a very difficult period of his city's history, was one of the leading figures in the Florence of Dante. An intelligent and on the whole flexible follower of St. Thomas, he has hitherto attracted attention because of the probable importance of his influence on the young poet's thought. He is, however, also worthy of study in his own right. His works are not brilliant; they reveal a clear mind rather than a profound one, more distinguished for learning than for originality. As a political commentator and theorist, however, he is capable of shrewd observation and analysis. He cannot be dismissed as a pedant; his efforts to apply Aristotelian principles to contemporary events show, in the midst of pious platitudes and conventional statements, evidences of an uncommonly realistic spirit. He dealt thoughtfully though hardly conclusively with the great problem of the relationship between secular and ecclesiastical authority; he was tough-minded as well as patriotic in his appraisal of his own city-state. The crises through which he lived sometimes shook, but apparently never destroyed, his robust Aristotelian optimism; they did, however, strengthen his conviction that all other human goods are subordinate to the *bonum commune* of peace. His writings provide a useful window into the life of late thirteenth- and early fourteenth-century Florence. They also come to grips with two of the gravest issues of his day: the claims of the Church to possess direct and universal temporal power, and the threat of faction to his own city and to the common life of its inhabitants.

It was natural that Remigio should take an interest in politics. He belonged to an influential family of *popolani* in Sesto San Pancrazio; his father Chiaro was one of the *Anziani* in the new government of the Primo Popolo in 1250; his brother Salvi was a member of the Council of Fourteen in 1280, in the time of uneasy truce between Guelfs and Ghibellines, and was chosen to represent the Arte della Lana as one of the priors of the crucial year 1282. Salvi was several times re-elected to this office,[1] and his three sons Chiaro, Girolamo, and Mompuccio also achieved the priorate; the latter were exiled by the Black Guelfs in 1302.[2] The religious profession of their uncle Remigio of course barred him from playing so direct a role in Florentine politics. Perhaps he served as one of the peacemaking friars in 1295.[3] Certainly his voice was heard often during these years on important occasions, when sermons were required for the reception of illustrious guests or the burial of well-known prelates or laymen.[4] He had excellent opportunities to observe his world, for the great convent of Santa Maria Novella was more a vantage point than a retreat, with its constant stream of visitors from all over Europe. Moreover, Remigio himself went at least twice to Paris and once to lecture on theology at the papal curia in Perugia. There was no reason for his view to be bounded by the walls of his own convent, or, indeed, of his own city.

Our information about these travels and about the external facts of his career is largely based on the entry devoted to him in the *Necrology* of Santa Maria Novella,[5] supplemented by a few documents and by references in his own writings. The date of his birth is unknown, but the *Necrology* tells us that he completed the course in

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[1] San Luigi, Ildefonso di, *Delizie degli eruditi toscani* **7**: 102; **8**: 24, 28, 32, 36, 38, 45, 57, 76, Florence, 1770. Davidsohn, Robert, *Geschichte von Florenz* **2** (2): 215. Berlin, Mittler, 1908.

[2] Mariani, Lorenzo Maria, *Priorista fiorentina* **1**: 7r. Florence, 1718; Davidsohn, *op. cit.*, **3**: 210–212, 1912.

[3] See Salvemini, G., *Magnati e popolani in Firenze dal 1280 al 1295*, 229, Florence, 1889, and *Conventi soppressi G.* 4. 936, ff. 355v–356r in the Biblioteca Nazionale of Florence.

[4] See the extracts printed by Salvadori, G., and V. Federici, I sermoni d'occasione, le sequenze e i ritmi di Remigio Girolami fiorentino, *Scritti vari di filologia a Ernesto Monaci*, 455–508. Rome, Forzani, 1901.

[5] The *Necrology* was begun in 1280. For the entry regarding Fra Remigio, see Orlandi, Stefano, *Necrologio di S. Maria Novella* **1**: 35–36, Florence, Olschki, 1955.

arts at Paris "in the first flower of his youth," was there received into the Dominican Order (apparently in 1267, if he remained in it until his death fifty-one years and ten months later), and shortly afterwards finished his studies and returned to Florence. Even before he was ordained priest, he was made lector of the Dominican *studium* in Santa Maria Novella, and held this office for more than forty-two years. He was sent again to Paris to read the *Sentences* and perfect himself in theology. It was a bitter disappointment to him that he did not receive the *magisterium* immediately, but finally it was granted him by Pope Boniface VIII in 1302. He was several times Diffinitor of provincial and general chapters of his Order, once prior of his convent, and once prior of the Roman province.[6] When he was older, according to the *Necrology*, poor health forced him to abandon some of his active duties and devote himself to writing. Of his industry in this regard we have ample proof.[7] His death occurred in 1319, and he was buried with honor in Santa Maria Novella.

The *Necrology* does not specify either the date at which he became lector or the date of his second journey to Paris. The former event must have been anterior to 1277, or Remigio could not have occupied his post for forty-two years; if we allow for the subsequent period of study in Paris and for the time Remigio spent in Perugia [8] we should probably push the year back at least to 1274. As for the second journey to France, it is fairly certain that this occurred before 1294, as there are sermons which prove that Remigio was in Florence during this and subsequent years.[9]

In one of his sermons Remigio called St. Thomas "the light of our eyes and the crown of our head"; two of the Angelic Doctor's sermons are included among his own works.[10] It is possible that Remigio heard his lectures during the last period of Aquinas' teaching at Paris (1269–1272). Whether from him or from others, Remigio there acquired the Aristotelianism which formed the basis of his philosophical and political thought.

As lector of the Dominican *studium* at Santa Maria Novella, which Grabmann believes was opened to laymen in time for Dante to have attended his lessons,[11] Remigio lectured on the *Ethics* of Aristotle as well as on the *Sentences* of Peter Lombard.[12] Moreover, in his prologue on the subject of knowledge in general, he glorified the human intellect in terms as fervid as those which Dante used in the *Convivio*.[13] Like the poet, he quoted the Aristotelian axiom that man as a rational creature desires the knowledge which is his perfection.[14] He pointed out a three-fold reason for this: (1) everything seeks its own perfection and the soul perfects its potentialities in so far as these are made active through knowing; (2) everything has an inclination towards follow-

[6] Remigio is listed among the masters of theology of Paris, "licentiatus per Bonifacium papam VIII anno domini MCCCII," in Stephanus de Salamaco et Bernardus Guidonis, *De Quatuor in quibus Deus Predicatorum Ordinem insignivit*, ed. T. Kaeppeli, O. P., *Monumenta Ordinis Fratrum Predicatorum Historica* **22**: 131, 1949. For his offices, see *Acta capitulorum provincialium provinciae romanae*, ed. Kaeppeli, *ibid.* **20**: 57, 105, 111, 128, 145, 154, 1941. For accounts of Remigio's life, see, in addition to the *Necrology*, Fineschi, V., *Memorie storiche che possono servire alle vite degli uomini illustri di S. Maria Novella*, Florence, 1790; Salvadori and Federici, *op. cit.*; and Orlandi, *op. cit.*, 276–285.

[7] For a list of the manuscript writings attributed to him (very few of which have been published), see Orlandi, *op. cit.*, 285–307. Some of the attributions are not certain. Orlandi also gives, in the note to 276–277, a valuable bibliography of secondary studies. The two most important codices, both of the fourteenth century and both in the National Library of Florence, are Conventi Soppressi C.4.940 and G.4.936. The first contains treatises; the second sermons (or outlines of sermons), together with theological and philosophical prologues. They will henceforth be referred to respectively as *C* and *G*.

[8] See Orlandi, *op. cit.*, 277.

[9] Salvadori and Federici, *op. cit.*, 457.

[10] *G*, 354r, 268v–271v. For references to St. Thomas's writings, see *G*, 192v and *C*, 54v. The relations between the economic thought of St. Thomas and Remigio have been studied by Capitani, O., La *venditio ad terminum* nella valutazione morale di San Tommaso d'Aquino e di Remigio de' Girolami, *Bull. dell'Istituto storico ital. per il medio evo* **70**: 299–364, 1959.

[11] Grabmann, M., Die Wege von Thomas von Aquin zu Dante, *Deutsches Dante-Jahrbuch* **9**: 1–35, 1925. *Cf.* Grabmann, *Mittelalterliches Geistesleben* **1**: 361–369, Munich, Hueber, 1926.

[12] There is a prologue devoted specifically to the *Ethics* in *G*, 344v–345r.

[13] Salvadori, G., *Sulla vita giovanile di Dante*, 109, Rome, Soc. Dante Alighieri, 1907, makes the much too sweeping statement: "Now he who, remembering the concepts, the terminology, the authorities, and the mode of reasoning of the *Convivio*, reads at least the first two among these prologues (that on knowledge in general and on the *Ethics*), will find everything there."

[14] Remigio, *G*, 341v; Dante, *Convivio*, I, i, 1. There is of course no way of proving Salvadori's assertion that Dante quoted this widely known axiom directly from Remigio. The relevant passage in Remigio's prologue is printed by Busnelli and Vandelli in their edition of the *Convivio* **1**: 87, Florence, Le Monnier, 1953.

ing its proper operation and "the proper operation of man, in so far as he is man, is to know"; (3) everything desires union with its *principium* and "the *principium* of our intellect, through which man is man, is God, to whom man is joined by knowledge." It is true, he says, that few men follow this natural end; misled by ignorance they seek lesser goods like riches, pleasures, and honors. But the knowledge available to the intellect is a superior good, for without it one cannot have the highest good, the uncreated, and with it one can have every created good. Such wisdom helps men to possess the four cardinal virtues, and culminates in theology and in the knowledge of God.[15]

This "proper operation of man," Remigio declares, should be pursued simply because of love for its object, wisdom, which is an end in itself, and should be loved singlemindedly, not from utilitarian considerations of wealth or reputation. The teacher ought to impart knowledge in the same way, though as Valerius says, there is no humility so great that it is not touched by the sweetness of glory.[16] The passage makes one think of Dante's words in the *Convivio*, inveighing against the false lovers of Lady Philosophy.[17] Remigio expresses the same ideal in a sermon, emphasizing the necessity of humility to reach that wisdom *que est optimus habitus intellectus*, and he compares the *sapientia terrena* of the avaricious with the cunning of the mole who understands well how to burrow under the ground but is blind when confronted with the stars.[18]

One may doubt that Remigio escaped entirely the vanities which he here condemns. Certainly, however, he is a good example of that other hard educational ideal of Dante's, that no true lover of wisdom confines himself to certain of her members, like him who only studies rhetoric or music for delight, and flees the other branches of knowledge, which are all parts of her beauty.[19] Whatever we may think of his originality, Remigio's versatility is astonishing. The Dominican lector produced not only volumes of sermons, Biblical commentaries, and philosophical and theological prologues; he also composed numerous treatises which embrace theology, philosophy, politics, economics, and general educational theory. A list of the contents of the codex (Conv. Sopp. C.4.940) containing only the treatises gives an idea of his range.

1. *Divisio scientie*. ff. 1r–7r.
2. *Determinatio de uno esse in Christo*. ff. 7r–11v.[20]
3. *De mixtione elementorum inmixto*. ff. 11v–17r.
4. *De modis rerum*. ff. 17v–70v (also in Conv. Sopp. E.7.938, Bibl. Naz., Florence).
5. *Quodlibetum primum*. ff. 71r–81v.[21]
6. *Quodlibetum secundum*. ff. 81v–90v.[22]
7. *Questio de subiecto theologie*. ff. 91r–95v.[23]
8. *De bono communi*. ff. 97r–106r.[24]
9. *De bono pacis*. ff. 106v–109r.[25]
10. *De peccato usure*. ff. 109r–124v.[26]
11. *De contrarietate peccati*. ff. 124v–130v.
12. *Determinatio utrum sit licitum vendere mercationes ad terminum*. ff. 130v–131r.[27]
13. *De mutabilitate et immutabilitate*. ff. 131r–135v.
14. *Speculum*. ff. 135v–154v.
15. *Contra falsos ecclesie professores*. ff. 154v–196v.[28]
16. *De misericordia*. ff. 197r–206r.
17. *De iustitia*. ff. 206r–207r.[29]

[15] *G*, 341v–342r. *Cf. Convivio* I, i, xi; II, iv, vii.

[16] *G*, 343r, 44r.

[17] *Conv.* III, xi.

[18] *G*, 197r, 201r.

[19] *Conv.*, III, xi.

[20] Ed. Grabmann, *Miscellania Tomista*, 257–277 (*Estudis Franciscans*, **24**, Barcelona, Oct.–Dec. 1924).

[21] Its subject, "utrum Deus possit facere quod materia existat actu sine forma," is the very question that puzzled Dante and caused him to interrupt his praise of philosophy. See *Conv.* IV, i, 8 and Renucci, P., *Dante disciple et juge du monde gréco-latin*, 48, Paris, Les Belles Lettres, 1954.

[22] This was delivered before the papal curia at Perugia, according to the *explicit*, apparently in 1304.

[23] For a table of contents of the first two quodlibets see *C*, 95v–96r.

[24] Dr. Lorenzo Minio-Paluello is preparing an edition of this treatise. See his article, Remigio Girolami's *De bono communi*, *Ital. Studies* **11**: 56–71, 1956, and Egenter, R., Gemeinnutz vor Eigennutz. Die soziale Leitidee im *Tractatus de bono communi* des Fr. Remigius von Florenz, *Scholastik* **9**: 79–92, 1934.

[25] For the text and an analysis see Davis, C. T. Remigio de' Girolami and Dante: a comparison of their conceptions of peace, *Studi danteschi* **36**: 105–136, 1959.

[26] Dr. Ovidio Capitani is preparing an edition of this treatise.

[27] The text has been published by Capitani, *op. cit.* 343–345.

[28] On this treatise see Grabmann, *Studien über den Einfluß der aristotelischen Philosophie auf die mittelalterlichen Theorien über das Verhältnis von Kirche und Staat*, *Sitzungsber. d. Bayer. Akad. d. Wissens. Philos.-hist. Abt.*, 1934.2, pp. 18–33 and Maccarrone, M. Il terzo libro della *Monarchia*, *Studi danteschi* **33**: 73 and *passim*, 1955.

[29] I hope soon to publish the text of this treatise.

De via paradisi. ff. 207r–352v.

Grabmann has justly remarked: "This manuscript reveals to us a variety and a disparity of content which we do not find in the other scholastic writers of that period." [30]

So remarkable was the breadth of Remigio's reading. Augustine and Aristotle were his great masters; in addition he quoted or cited Cicero (*De Republica, De Amicitia, De Officiis, De Senectute, Tusculan Disputations,* and some of the orations), Virgil, Horace, Lucan, Ovid (*Fasti, Remedia Amoris, Tristia*), Martial, Suetonius, Seneca (*De Clementia, De Beneficiis, De Naturali-*), Valerius Maximus, Aesop, Macrobius, and Boethius at least of Avicenna, Averroes, Alfraganus, Ptolemy's *Almagest,* as well as Christian writers like Boethius, Lactantius, Gregory the Great, Gregory of Nazianzus, Cyril of Alexandria, Augustine, Ambrose, Jerome, Chrysostom, Bede, Richard, Hugh of St. Victor, Peter Lombard, and Thomas, and the inevitable medieval texts from Pseudo-Dionysius, the *Disticha Catonis* and the *Nauasis.* Even such a summary list as this, derived from only a part of his works, indicates his knowledge of classical authors and his admiration for the Roman past. Moreover, his attitude toward the texts he quoted, though not "humanistic" in the sense of an attempt to recreate the Roman world, was at least unusual for his time in its self-consciousness. At the end of the *Tractatus Falsos Ecclesie Professores* he made the following confession that sometimes he abridged quotations and used them in a way different from that intended by the author. He added that he did not think this was a very grave matter since Holy Writ as well could be interpreted in various senses according as they did not contradict faith, knowledge, and good morals. The surprising thing is not that Remigio did this but that he conceived an apology to be necessary. He also admitted that he did not always go back to the original works for the quotations but sometimes took them at second-hand from other writings.[31] Yet his references are usually correct and his quotations exact; they are, moreover, numerous cross-references to other writings scattered through his works.[32]

All this reveals an unusually conscientious scholarship which was one of the chief merits of the careful lector of Santa Maria Novella; in his manipulation of texts he remained aware of their provenance and original meaning.

Remigio was an admirer of Roman virtue as well as of Roman literature. Although he apparently never, with Dante, called Florence "the most beautiful and famous daughter of Rome,"

Grabmann, Fra' Remigio de' Girolami O. P., Lolo di S. Tomaso d'Aquino e maestro di Dante, Scuola Cattolica **53**: 361, 1925.

, 196r.

These cross-references are also to be found in *G,* in another early fourteenth-century codex in the teca Nazionale of Florence, Conv. Sopp. D.1.937, ining more sermons of Remigio, those in honor of the saints. This characteristic of the manuscripts, together with their careful accuracy, and the fact that they all contain numerous marginal corrections and insertions of new material in the same hand, makes it likely that the Dominican himself supervised the collecting and copying of his works. This impression is strengthened by the statement at the end of the entry in the *Necrology* about the lengthy and useful volumes compiled and produced by Remigio after ill-health had curtailed his active teaching and preaching. It is confirmed by a note in both volumes of sermons: "Expliciunt sermones de sanctis per totum annum fratris Remigii florentini, ordinis predicatorum magistri in theologia. Inter quos, sicut dixi de sermonibus de tempore, sunt aliqui accuratius editi. Multi vero sunt defectuosi et diminuti et nonnulli sunt in ordine partium et membrorum interdum transpositi et aliquando prius dictorum replicativi propter diversa impedimenta. Sunt etiam aliqui licet pauci ex dictis aliorum inmixti. Quos quidem diligens lector advertat et caritative corrigat et emendet . . ." (Conv. Sopp. D.1.937, ff. 371r–v; for the same statement, with slight verbal alterations, see *G,* 242r).

An examination of these three manuscripts seems to make it clear that one hand, A, wrote the main text of the two volumes of sermons and the major part of the text of the volume of treatises (beginning with f. 74v), as well as a few of the marginal insertions. Another hand, B, probably Remigio himself, supplied the rest of the marginal material, and the brief *De nomine usure* written in on the last pages of *G.* V. Federici, in a note to the already cited article of Salvadori (pp. 471–475), analyzes this last manuscript, and says that though at first glance it seems to be the work of two hands, further study convinced him that the whole volume is an autograph. The discrepancies in the writing may be explained, he believes, by the cursive character of most of the marginal material, and by the fact that Remigio added it late in life. But as F. Salvatori, *Due sermoni inedite di s. Tommaso d'Aquino,* 10, Rome, 1912, has observed, there is a marginal note in *C,* 231v: "hic fuit erratum per scriptores." This note and the marked differences in calligraphy find a much more natural explanation if Remigio is thought to have had scribal help in the production of these volumes. It should also be noted that none of the marginal material added by B refers to events later than the date of Remigio's death. It seems to have been inserted at different times with no attempt at scribal neatness; a look at the sermon for the death of Louis X in 1316 (*G,* 387v–390v), where it was necessary to skip one margin already occupied by an earlier sermon, reinforces the feeling that it was Remigio himself who emended his works.

he had a great regard for the imperial city which Florentines considered their mother. In the *De Bono Communi,* written when the spirit of faction in Florence was at its height, he invoked the heroes of the pagan republic as examples to shame his fellow citizens. Like Dante in *Convivio* IV, v, he took most of his examples from St. Augustine's *De Civitate Dei,* but also like Dante, he changed very considerably Augustine's ironic moral. The Christian Father remembered the Roman heroes with a reluctant admiration, but for him their sacrifice was vain and based on human pride; its only value was to incite Christians to a higher sacrifice for the true *patria* of Heaven. Remigio, however, expressed unmixed respect for such figures as Lucius Valerius, Cincinnatus, Fabricius, Torquatus, and Marcus Regulus, who "cared more for the common good than their own." Remigio included Cato in this list, though he did not, like the poet, make the Roman a martyr to liberty. Instead, he repeated the stricture of Augustine on his suicide and at the same time quoted the more charitable opinion of those who thought that his bitterness over Caesar's triumph came not from personal motives but from patriotism, from his feeling that "on account of this the *res publica* had fallen into great danger." [33]

The Dominican lector also had high praise for Cicero, though he did not mention his name among this particular group of heroes, as Dante was to do in the *Convivio.* For Remigio, Cicero was the perfect type of the intelligent patriot, who tried to conciliate faction without joining it, and who had a just conception of nobility. In the course of a long harangue against the Blacks and the Whites, Remigio quoted, from the apocryphal work *Invectiva contra Sallustium,* Cicero's defense against those who accused him of disloyalty to his friends: "quantum quisque rei publice studuit, tantum mihi fuit aut amicus aut inimicus." [34] Remigio's comment on this was: "Et talis duplicitas optima est. Tullius enim ut gentilis optimus fuit apud latinos." [35]

In the *De Via Paradisi* Remigio quoted again from the *Invectiva* to support his definition of nobility, a definition also maintained by Dante in the *Convivio* and by Brunetto Latini in the *Tresor.* Like them, Remigio declared that true nobility could be attained by those who followed virtue no matter how humble or wretched their birth: "Unde Tullius dicit in *Invectiva contra Salustium:* 'Noli mihi obiectare antiquos viros. Satis enim est me meis gestis florere quam maiorum opinione niti, et ita vivere ut ego sim posteris meis nobilitatis initium et virtutis exemplum.'" [36] For the Dominican these noble deeds consisted largely in serving the common good and avoiding useless civil strife. He admired Julius Caesar's magnanimity in ignoring and forgiving his enemies.[37] He also, like Dante, thought of Augustus' reign as the golden age, the plenitude of time in which war and political disorders were stilled; he said that Christ had chosen this time as the appropriate hour for His birth.[38] If Dante, as seems probable, was acquainted with the lector of Santa Maria Novella, it is not unlikely that Remigio's idealization of Rome helped to change the young poet's earlier, unfavorable attitude to that city and to convince him that there was a providential purpose in the establishment of the Roman peace.[39]

This supposed peace and order of Rome, however, must have seemed very far away in thirteenth-century Florence. Such meager comments on contemporary events as can be gleaned from Remigio's sermons show him to be preoccupied by the problem of faction. In what is evidently an early sermon (perhaps delivered soon after the futility of Cardinal Latino's settlement had become apparent), he analyzed astutely the main divisions of Florence: political, economic and social, and religious. In the first place, he said, the Guelfs hated the Ghibellines because they would not yield, and the Ghibellines the Guelfs because they feared expulsion from the city. In the second place, the *artifices* complained of the avidity and oppression of the *magni,* and the *magni* of the insolence of the *artifices.* And in the third place, clerics and laymen exchanged bitter

[33] Excerpts from the passage are quoted by Minio-Paluello, *op. cit.,* 68. For a fuller discussion see my book, *Dante and the idea of Rome,* 43–53, 82–84, Oxford, Clarendon Press, 1957.

[34] Ed. by A. Kurfess, *Appendix Sallustiana* 2, Leipzig, Teubner, 1950.

[35] *Speculum, C,* 154r.

[36] *De via paradisi, C,* 270v. For the quotation see Kurfess, *op. cit.,* 11.

[37] *G,* 48v, 233r.

[38] *G,* 12v; *cf.* Conv. Sopp. D.1.937, 43r.

[39] Minio-Paluello, *op. cit.,* 57 (in note) observes that it "is not safe to speak of Dante as a pupil of Remigio as Salvadori and others do; but it is difficult to believe that Dante should not have been in touch with the best centre of philosophical studies which Florence could provide, at the time of his philosophical 'conversion.'" It is also difficult to believe that he was entirely unacquainted with the historical views of one of the best-known preachers and philosophers in Florence.

and at least partially justified accusations of dishonesty, usury, fornication, sloth, gluttony, and pride. Only Christ Himself, said Remigio, could unite such a city; the somber conclusion touched a note of pessimism that he would return to again and again during the difficult years ahead.[40]

Another interesting passage is unfortunately fragmentary but seems to reveal Remigio's bias against the Ghibellines and his sympathy for the Guelfs and for the economically productive classes of the people: "Item quod ghibellini dicuntur leo, guelfi vitulus, quod est animal immolatitium, artifices et populares oves, quia innocentes et utiles et quia omnes laborant de ovibus, scilicet lanifices cum omnibus artificibus sibi subiectis. . . ."[41] It is a strange figure, but perhaps an appropriate one for a member of a family prominent in the Arte della Lana.

The struggle between Guelfs and Ghibellines, between two factions of the magnates, was not the only important source of tension in late thirteenth-century Florence. Just as significant was the battle for economic power among the groups and individuals referred to by Remigio under the general headings of *magni* and *artifices;* it was produced by immigration into the city, the scramble for fortunes, the inevitable rise of new men, and the oppressive measures introduced by the ruling oligarchy to maintain its power. For the most part Remigio, as a good cleric and a lover of peace, looked on the new popular agitations with conservative disapproval. He regarded Giano della Bella as a troublemaker and lamented the dissension which arose in Florence after his expulsion.[42] He naturally condemned usury[43] and was alarmed by the all-engrossing passion of his fellow citizens for amassing wealth. *Radix malorum cupiditas est,* he said with Dante,[44] and he was aware that it led not only to damnation in the next world but to civil strife in this.

It was not, he asserted, that money in itself was bad. On the contrary it might be argued that this *terra condensata* could be used to acquire all goods. One could become noble, with money, by marrying into the nobility; one could become healthy by buying precious medicine; one could become beautiful through the purchase of expensive clothes and cosmetics and ornaments; one could become popular by entertaining friends; one could become learned by hiring teachers; one could become a prelate by "honors" paid to other prelates; one could become absolved from sin by the practice of charity. As Ovid said, "In pretio pretium nunc est, dat census honores, census amicitias, pauper ubique iacet." And Horace was right to declare that "Virtus, fama, decus, divina humanaque pulcris divitiis patent." These goods were real enough if men could possess riches without loving them. But in actuality they were usually false goods, for wealth inevitably tended to disturb friendship, produce cupidity, foster idolatry, and encourage a luxurious and sinful life. Remigio quoted Aesop's fable of the city mouse and the country mouse, and also told of a certain *miles ditissimus de Tuscia* who, dying, cried out in disillusionment: "O floreni mei, non succurreretis mihi."[45] The security given by wealth was illusory and brief.

When the Dominican looked at the growing prosperity of his own city, his verdict was similar, though his pride in Florentine power was also very evident. God had given Florence, he said, seven singular gifts: abundance of money, a noble coinage, abundance of population, a civilized way of life, the wool industry, skill in the production of armaments, and a vigorous building activity in the *contado.* These gifts, if used properly, brought glory to Florence; if used improperly, they blinded her citizens with false pride. Remigio's eulogy of the florin showed the ambivalence of his attitude; he praised its "nobility" for three reasons: it was made of the best gold, it was decorated on one side with the image of John the Baptist and on the other with the Florentine lily, and it was accepted through-

[40] *G,* 76r–v: "Fracta est civitas magna in tres partes." Una fractio est quia Guelfi dicunt male de Ghibellinis quod non cedunt, et Ghibellini de Guelfis quod expellere eos volunt. Alia fractio est quia artifices dicunt male de magnis quod devorantur ab eis, quod proditiones committunt, [76v] quod bona inimicorum defendunt, et huiusmodi, et e contrario magni de artificibus quod dominari volunt et nesciunt quod terram vituperant, et huiusmodi. Tertia fractio est inter clericos et religiosos et laycos, quia de laycis dicunt quod sunt proditores, quod usurarii, quod periuri, quod adulteri, quod raptores, et verum est de multis. Et e contrario layci dicunt quod clerici sunt fornicarii, glutones, otiosi, quod religiosi raptores, vanagloriosi, et de aliquibus verum est. Supradictas istas fractiones potest consolidare et unire solus Dominus noster Iesus Christus per gratiam suam, quia ipse est pax nostra qui fecit utraque unum, ut dicitur Eph. 2.

[41] *G,* 42r.

[42] *G,* 355r–v; Salvadori, *op. cit.,* 482.

[43] See above, notes 26 and 27.

[44] *G,* 55v.

[45] *De via paradisi, C,* 216v–217v, 323v; cf. 201r–202v, 326r–v.

out the world, carrying the name of Florence even among the Saracens.[46] This patriotic note was balanced by the warning, "Hac igitur nobilitate excecatur elatus et vanagloriosus," though Remigio's feeling that Florence was a chosen city was evidently strong.

More and more, however, his pride in her growth must have been clouded by grief over her factional wars. In the treatise *De Bono Communi*, evidently written soon after the expulsion of the Whites in 1302, he lamented the disorders in Italy and in Florence and said that even the name "Florentia" was no longer appropriate; the corruption "Firenze" was better, for just as the French said "fi, fi" when their nostrils were wrinkled by an evil smell, so the citizens of Florence must be aware that the perfume of her fame had been transformed into the evil stench of infamy.[47] In another treatise, entitled *Speculum* and apparently written at an earlier stage in the struggle, he included a long and impartial denunciation of the vices of the Blacks and the Whites. Here Remigio was the preacher rather than the political theorist; in the whole course of his exhortation not one word of practical analysis or advice found a place. He began by lamenting the fact that there had never been so profound a division in the city: "nunquam tanta disiunctio seu contrarietas voluntatum fuit in Ghibellinos et Guelfos vel inter plebem et ingenuos quanta nunc existere videtur inter albos et nigros." He went on to declare, with overingenious and protracted plays on the words *album* and *niger*, that both sides were guilty of such sins as pride, avarice, luxury, and obstinacy, and that neither side was willing to live up to the Augustinian definition of justice, the readiness to render to each his right. Instead, the wills of the citizens were alienated from God, from reason, and from each other. Reform of Florence should thus be an individual matter; Remigio showed none of Dino Compagni's interest in the history of the struggle, or Brunetto Latini's concern with the governmental functioning of the city. He did however, touch the vital point. He knew that however well-constructed or ill-constructed a government may be, the essential force binding any political community together must come from "the union or conjunction of hearts, that is of wills willing the same thing" and forming the peace, the *summum bonum*, of the state.[48] In the *De Bono Communi* Remigio emphasized the rational aspect of this concord of wills by calling it a union of minds, a "totalitas totius integralis non quidem ex partibus corporalibus directe et principaliter sed ex partibus rationalibus."[49] The citizen should be a patriot, filled with rational love for his city. As a man he is a reasoning, and therefore a political, animal; the community is the whole which gives life to him, its part. Just as the hand or foot is dependent upon the entire human body, so the individual man, *qua* part, is dependent upon the community. The hand severed from the body is not really a hand; the individual severed from the city is not really a man, for he is born to live with companions, and that person who chooses solitude must, as Aristotle says, be either god or beast, must either rise above the human condition or sink below it. He may be a contemplative inspired by superhuman love for God;

[46] *G*, 89v–90r: Deus autem contulit isti civitati septem bona quasi singularia quibus homo si male utatur, sicut frequentius accidit, excecatur, si vero debite, illuminatur, scilicet habundantiam pecunie, nobilitatem monete, multitudinem populi, civilitatem vivendi, opificium lane, et artificium armature, et domificationem contrate in comitatu vel in districtu. . . . Circa primum notandum est quod pecunia, siquis utatur ea avare, excecat avarum. Circa secundum nota quod nobilitas monete apparet ex triplici parte, scilicet ex parte materie, quia aurum tarenorum est bonum sed aurum augustalium est melius sed aurum florenorum est optimum, et ex parte sculpture, quia ex una parte habet Beatum Io. Baptistam de quo Dominus dixit Mat. xi: Inter natos etc., et ex alia parte habet lilium, quod est res magne excellentie, unde et Christus et mater sua lilio conparantur in Can. [5:13]; tertio ex parte cursus quia quasi per totum mundum etiam inter Saracenos currit. Hac igitur nobilitate excecatur elatus et vanagloriosus. [90r] Circa tertium nota quod ex multitudine populi excecatur confidens in ipsa. . . . Sed ex ea illuminatur confidens in Deo quasi auctore multitudinis dante instrumentum virtutis. . . . Circa quartum nota quod civilitas excecat si declinet ad malum, puta luxuriam, gulam, predam, homicidium, periurium, etc. . . . Circa quintum notandum est quod opificium lane excecat siquis utatur hoc in ypocrisim quasi nolens inalum lucrum facere usurarum et huiusmodi sed sub hoc velamine commictens periuria, fraudes, usuras, et huiusmodi. . . . Circa sestum nota quod armatura excecat siquis utatur ea contra Deum . . . sed illuminat siquis utatur ea contra inimicos Dei. . . . Circa septimum nota quod domificatio districtus excecat inprovidum qui scilicet totum vel magnam partem suarum divitiarum ponit in huiusmodi domificatione sed illuminat providum qui scilicet, cum sit ditissimus, solatium sibi accipit de parte aliqua.

[47] *De bono communi*, *C*, 101v, quoted by Minio-Paluello, *op. cit.*, 60.

[48] *Speculum*, *C*, 146r–154r.

[49] *De bono communi*, *C*, 104v. *Cf.* Egenter, *op. cit.*, 84.

he may be a recluse (or a criminal) who scorns his brother; his normal state, however, is that of a citizen bound by ties of natural and rational affection to his fellow citizens and to the community of which they are the parts.

These basic concepts are of course Aristotelian. Like Aristotle, Remigio holds that man is made for society, that the purpose of government is to promote a self-sufficient and virtuous life in a community, that the welfare of this community is superior to the welfare of the individual, and that the purpose of law is to pursue the common good.[50] Remigio, impelled by his hatred of faction, pushes these postulates very far in attempting to show why each particular man should subordinate his self-will to the needs of the state. It is natural, he declares, not only that the citizen should love his community, but that he should love it more than himself. Its good is higher than his, its beauty is greater than his, its existence is superior to his, its beatitude is more comprehensive than his. It offers him material goods, intellectual development, and spiritual growth. The first are brought together by men of many crafts and professions; the second is made possible by learned teachers and stimulated by friendly discussion; the third is brought closer through the practice of social virtues. Only the fact that men's minds are darkened by sin makes it difficult for them to live together in peace. For Remigio the old Augustinian idea of the state as a restraint on evil is still important, but it is overshadowed by the more positive and optimistic Aristotelian conception of the state as an opportunity for virtue.

But what if the interest of the community and the interest of the individual come into conflict? Remigio's answer is that this conflict is only apparent and never real, for "in the good of the whole the good of the part is without any doubt included."[51] Why did the ancient Romans offer up their lives for their city? Principally because of the general good, but secondarily because of their own private good, since the *summum bonum* of the citizen is the possession and practice of virtue. Even more readily should men be prepared to sacrifice their material riches to the commune, for on its continued existence everything else depends. This, again, is only the secondary benefit; the primary end is simply the maintenance of that whole which the parts love more than themselves and to which they are more closely joined than to themselves.[52] Such a sentiment of devotion is analogous to that feeling which the Christian should have for God. On a subordinate level God is loved in order to avoid Hell and win salvation. On the highest level He is loved for Himself by the soul which spontaneously yearns for union with Him in Paradise.

But are there no occasions when the welfare of the individual should be put before the welfare of the commune? Remigio suggests two dilemmas. Is it not better, he asks, to watch the community sin than to sin oneself? And would not a citizen prefer to see his commune, and indeed the whole world, in Hell rather than go there himself? Surely here the part may be forgiven for thinking of its own good rather than that of the whole.

The dilemmas, however, are declared to be false. Remigio answers the first question by asserting that sin can never be tolerated since it is an offense against God and therefore worse than an offense against man or the commune. By refusing to do evil the citizen does not say that he is superior to the commune; he says that the commune is inferior to God. Besides, he must always deplore the evil done by his city, as it necessarily includes him as well. Sin is to be condemned absolutely and should not be committed on account of any love. As for the second question, he asserts that the pain of damnation presupposes guilt, or injury to God, which is worse than any injury to the commune. The Christian must abhor the guilt, but he must rejoice in the punishment which chastises it, since this, but not guilt, comes from God. Certainly the citizen should be willing to accept any punishment which does not involve previous guilt, rather than see a like penalty visited on his city.[53]

[50] For a fuller discussion see Davis, Remigio de' Girolami and Dante.

[51] *De bono communi*, C, 103v.

[52] *Ibid.*

[53] *Ibid.*, 104v. Remigio says: "Et ideo est ibi [in culpa] offensa Dei quem tenemur preamare toti mundo et propter amorem ipsius gaudere de pena inflicta etiam infernali quantacumque communi a Deo propter offensam ipsius Dei . . . Si autem quantacumque pena posset esse *sine culpa*, ex virtute amoris ordinati *homo* deberet potius ipsam velle patitum immunitate communis, quam quod commune suum ipsam incurreret cum immunitate sui, inquantum est pars communis." But *cf.* Kantorowicz, E. H., *The king's two bodies*, 480 and note, Princeton, Princeton Univ. Press, 1957.

The last argument is plainly a rhetorical device, a reiteration by means of hyperbole of the lengths to which the individual should be prepared to go in service to his commune. Presupposing as it does two impossibilities (the commune in Hell and one citizen by taking its place able to redeem it), its exaggeration seems half-playful. It is similar in this respect to another of Regimio's sayings, that the Pope can do more than the Devil and more than God; he can do more than the Devil because he can do good, and more than God because he can sin. Yet Egenter, in his interesting study of Remigio's doctrine of the common good, seems to take the argument literally, and Kantorowicz, in his stimulating discussion of "man-centered kingship" in Dante, says that "Remigio advocated the *eternal* death of the soul . . . for the sake of the temporal fatherland." [54] The Dominican, however, observes that guilt, which alone can produce damnation, would offend a higher power than the commune. His rhetorical device may appear far-fetched, but it presents no real problem; there are other far more questionable points in his treatise.

One of them has been observed by Egenter, who says that Remigio often does not take account of the fact that there is an essential difference between a part which has no existence outside an organic whole and an individual who has a very real existence outside the associative unity of the state.[55] Kantorowicz is right to complain that "for the sake of the *corpus mysticum* of the city Remigio strangely devaluates the physical individual which alone, according to Genesis, was created in the likeness of God." [56] Moreover, any rational autonomy which the individual possesses is swallowed up in the state; it is true that Remigio repeatedly quotes the prohibition in Exodus against following the mob to do evil; on the other hand he says that only supernatural love enables a man to follow God when the commune rebels against him.[57] The private judgment of the individual, apparently, has little force; in the natural and rational realm the state will override it. On a practical level, we are given little reason to trust Remigio's equity in applying his definition of that justice which is supposed to give to every man his right. In the *De Bono Pacis* he recommends the cancellation of all claims, just and unjust, on the part of those who have suffered losses in the factional disorders of Florence. Even the grievances of the Church should be disregarded, he declares, even the threat of papal excommunication should be ignored, if the cause of peace, i.e. of civic tranquility, can thereby be served. Remigio hopes that the commune will be generous in particular cases of suffering, especially where the spoils are still in the hands of those who have wrongly taken them. Nevertheless, for him the decisive consideration is the interest of the state and not the claims of justice.[58]

There is, in the *De Bono Communi*, a rather chilling evidence of bad taste in Remigio's description of the superior beauty of the city. He says that just as a splendid palace contains of necessity certain *loca fetida*, like stables and privies, which are essential to the proper functioning of the whole, so every community contains poverty and misery. Yet each individual, however wretched his personal condition may be, ought to remain quietly in his own place, content that he is helping his city to flourish.[59] In summary, Remigio is much more explicit about the duties which the citizen owes to his community than about the duties which the community owes to its citizens. His corporationalism is extreme,[60] and yet his analysis of exactly how each citizen benefits from his membership in the larger whole is vague. Perhaps it is going too far to call him, with Kantorowicz, "that curious thomistic proto-Hegelian." [61] He did not put forward a religion of the state: life in community was an aid to reaching God, but by no means a substitute for God; its peace was valuable primarily as it led on to the spiritual peace which was not human

[54] Egenter, *op. cit.*, 89, and Kantorowicz, *op.* and *loc. cit.*

[55] Here Remigio diverges from St. Thomas's teaching: "ratio partis contrariatur rationi personae" in his commentary on the *Sentences*, III, dist. v, quaest. 3, art. 2. I owe this quotation to the courtesy of Dr. Thomas Prufer. Egenter discusses the position of St. Thomas at some length, *op. cit.*, 79–84.

[56] Kantorowicz, E. H., *Pro patria mori* in mediaeval political thought, *Amer. Hist. Rev.* **56**: 489, 1951.

[57] *De bono communi, C*, 102v–103r.

[58] *De bono pacis, C*, 106v–109r. The date of this treatise is 1304.

[59] *De bono communi, C*, 99v.

[60] Lagarde, Georges de, Individualisme et corporatisme au moyen âge, *L'organisation corporative du moyen age à la fin de l'ancien regime* **2**: 40, Louvain, 1937 (*Recueil de travaux publiés par les membres des Conferences d'Histoire et de Philologie*, ser. 2, vol. 44), says justly of Remigio: "on peut difficilement trouver un anti-individualisme plus accusé."

[61] Kantorowicz, *The king's two bodies*, 479.

but divine.[62] On the other hand, the implications of what Georges de Lagarde hails as "le mot magnifique de Girolami: 'Si non est civis non est homo,'"[63] are disquieting. It is easy to see why Remigio loathed faction and viewed it as private egotism writ large, but it is also plain that he was led to a dangerous idealization of the state by the violence of his reaction against disorder.

One must be careful, however, not to credit the Dominican with any modern concept of political sovereignty. He did not, at least in theory, regard the commune as an autonomous "state." The term itself is anachronistic even when we are considering Remigio's attitude towards Florence, and outside this narrow field it becomes positively misleading.[64] Though he seems never to have dealt with the problem of whether Florence recognized a temporal superior, he certainly regarded his commune as only one subordinate unit in the great hierarchy of "mystical bodies" which made up Christendom.[65] He also said that though the good of the commune was to be preferred to the good of an individual citizen, the good of a province, which contained many cities, of a kingdom, which contained many provinces, and of the universal church, which embraced the whole world, should arouse correspondingly greater love.[66] When he discussed, in the *Contra Falsos Ecclesie Professores,* what we should call the question of church and state, he did not use abstract terms of contrast, neither *sacerdotium* and *regnum* nor *ecclesia* and *imperium* nor *papatus* and *imperiatus.*[67] Instead he tried to determine the limits of the power of the Pope, Vicar of Christ and earthly head of the *ecclesia universalis.* The word *ecclesia* used in this traditional sense was not limited to clerics; it comprehended laymen as well and enfolded in its bounds the whole vast medieval hierarchy of spiritual and temporal authorities. It extended, said Remigio, from the height of heaven (Christ) to the depths of Purgatory; it included the earth and all the islands of the sea. In it, as in the sky, God had placed two great lights: the Pope, to rule over the day, that is over *spiritualia;* and secular rulers, to rule over the night, that is over *carnalia.* This was the gloss of Pope Innocent III to Genesis 1: 16–17; he was right to say, affirmed Remigio, that as far as the sun surpassed the moon in brightness, so far did the Pope surpass all other rulers.[68]

To this hierarchy of authorities there corresponded, for Remigio as for Aquinas, a hierarchy of laws, divine, natural, and positive, with the last subdivided into canon and civil. Natural law and canon law proceeded from divine law, and civil law from natural law; unjust enactments were not law but iniquity.[69] In his brief treatise on justice Remigio made a slightly different classification: law was twofold, innate and positive; positive law was divine and human; divine law was old and new; human law was civil and ecclesiastical; civil law was imperial and munic-

[62] For this distinction between the two ends of man, and the subordination of one to the other, see *G,* 358r–v.

[63] Lagarde, *La Naissance de l'esprit laïque au déclin du moyen âge* **4**: 181, Paris, Éditions Béatrice, 1934–1946.

[64] So far as I know, Remigio never uses the word *status* in a political sense. See *Contra falsos ecclesie professores, C,* 160v for its use in the sense of "estate," contrasting *status clericalis* with *status laicalis.* In the commentary of Aquinas and Peter of Auvergne to the *Politics* the word also means "government": *status popularis, status paucorum, status optimatum,* though not yet "state"; see Kantorowicz, *The king's two bodies,* 271, n. 235. Remigio used this commentary, but when he discussed Aristotle's views on the various constitutions he employed general terms like *principatus* or *gubernatio* and specific terms like *tymocratia* and *aristocratia* to carry this meaning. (*De via paradisi, C,* 208v).

[65] There is an excellent discussion of the origin and use of the term *corpus mysticum* (which referred first to the Eucharist and then to the Church and then to political communities as well) in Kantorowicz, *The King's two bodies,* 193–232.

[66] *De bono communi, C,* 97r.

[67] See Stickler, A. M., Sacerdozio e Regno nelle nuove ricerche attorno ai secoli xii e xiii nei decretisti e decretalisti fino alle decretali di Gregorio IX, *Misc. Hist. Pontificiae* **18**: 1–26, 1954, and Ladner, G. B., The concepts of "Ecclesia" and "Christianitas" and their relation to the idea of papal "plenitudo potestatis" from Gregory VII to Boniface VIII, *ibid.,* 49–77, for discussions of the first four of these terms. The last two are used by Dante, *Monarchia,* III, xii, 6.

[68] *Contra falsos ecclesie professores, C,* 156v–157r.

[69] *Ibid.,* 164v: "Ius autem triplex est, scilicet divinum, naturale et positivum, et hoc duplex, scilicet canonicum et civile. Ius autem naturale proficiscitur a iure divino, sicut etiam omnis natura procedit a Deo a quo etiam procedit et ius positivum canonicum. Ius vero positivum civile a iure naturali procedit, et ideo ubi ius positivum discordat a iure divino vel naturali non est dicendum ius sed potius iniquitas et iniustitia." *Cf. G,* 352v: "Iustitia enim dicitur ex hoc quod concordat legi scripte. Si ergo lex humana statuta sit secundum rationem rectam, tunc talis iustitia non contrariatur divine. . . ."

ipal.[70] In regard to the last division, Remigio did not say who had legislative power or what the relation was between the law of a city and the law of the empire. Nor did he make clear where the laws of a kingdom, for example, France, could be fitted into this scheme, although elsewhere he quoted with approval from Innocent III's *Per Venerabilem*, and said that compared to the French ruler all other kings were *reguli;* he could be called the King just as Aristotle was called the Philosopher, Paul the Apostle, Virgil the Poet, and Mary the Virgin.[71] In his discussion of justice, Remigio only sketched a general picture of the relationships joining together the various laws, a picture which, except for that interesting reference to the *lex municipalis*, was traditional and not subject to dispute.

The defining of the authority of the Pope over the universal church, however, was a much more controversial matter. Remigio attempted to show that secular jurisdiction was distinct from ecclesiastical, and to demonstrate, in opposition to current hierocratic views, that the Pope did not have authority over laymen "principally and directly in regard to temporalities."[72] His task was made harder by his eagerness to pay all legitimate honor to the Vicar of Christ. His initial concessions to the hierocratic point of view were so great that the independence which he tried to claim for the secular power was at best limited and at worst illusory. Only by a rather extended analysis can one hope to do justice to the subtleties of his doctrine of the *potestas indirecta* and to recognize, without exaggerating, its final inconclusiveness.

In this field, as in the rest of his political theory, Remigio revealed himself to be under the influence of the Thomistic interpretation of the Aristotelian political tradition. But if his exaltation of the city was far more extreme than that of Aquinas, his analysis of the relationship between the temporal and spiritual powers was perhaps more moderate. In spite of St. Thomas's emphasis on the distinction between them, the tendency of the latter's thought was hierocratic. He made clear in the *De Regimine Principum* that all society was ultimately directed to the end of salvation; in the *Summa Theologiae* he said, quoting Gregory of Nazianzus, that secular power was subject to spiritual as the body to the soul and there was consequently no usurpation of right if the spiritual power intervened in appropriate cases in temporal things; at the end of his earlier commentary on the second book of the *Sentences* he declared that in the person of the Pope both powers were joined.[73] But these were only hints, out of which later theorists might, if they wished, draw the consequences.

Aristotelian philosophy could, in fact, as de Lagarde has observed, lead medieval political thought in either of two directions. In the hands of hierocrats it could facilitate a "reductio ad unum": *one* origin for ecclesiastical and secular authority, *one* supreme end for man, *one* human head for *one* carefully articulated structure embracing all subordinate powers.[74] Had not Aristotle said "Omnia que sunt unius generis reducuntur ad unum, quod est mensura omnium que sub illo genere sunt; sed omnes homines sunt unius generis; ergo debent reduci ad unum"?[75] Giles of Rome and Ptolemy of Lucca were among the theorists who developed this line of thought. On the other hand, the rigor of the principle together with Aristotle's emphasis on the naturalness of the state could encourage an effort to place the origin and end of human society outside the order of redemption, with only a very qualified subordination to the ultimate authority of the Church, as in the writings of John of Paris and Dante. Remigio fell between the two positions, and to some extent, despite his valiant attempt to achieve a compromise, he had the worst of both worlds.

Judged by the standard of contemporary writers like Giles of Rome and Ptolemy of Lucca, the lector of Santa Maria Novella was not an hierocrat. Though he accepted the *reductio ad unum* of all men to the Pope,[76] he tried to minimize its

[70] *De iustitia, C*, 206v–207r: "Lex autem est duplex, scilicet innata et positiva. . . . Positiva autem est duplex, scilicet divina et humana, et sic est duplex iustitia. Divina autem est duplex, scilicet vetus et nova. Humana autem est duplex, scilicet civilis et ecclesiastica. Civilis autem est duplex, scilicet imperialis et municipalis."

[71] *G*, 387v, 390v, in margins (sermons for the deaths of Philip IV and Louis X). I hope soon to publish the text of both sermons.

[72] *Contra falsos ecclesie professores, C*, 161r.

[73] These views of St. Thomas are discussed by Grabmann, *Studien*, 12–14, and Maccarrone, "Potestas directa e "potestas indirecta" nei teologi del xii e xiii secolo," *Misc. Hist. Pont.* **18**: 39–40, 1954.

[74] See on this de Lagarde, *La naissance* **3**: 167–169.

[75] Dante quotes this argument in the *Monarchia*, II, xii, 1, referring to *Metaph.* X, i, although he denies that the measure of all men is the Pope. Maccarrone, il terzo libro, 93, observes that it is the guiding principle of Boniface VIII's *Unam sanctam*.

[76] *Contra falsos ecclesie professores, C*, 160v.

sequences. Their purpose was exactly the osite. Giles, for example, in the *De Ecclesia Potestate*, declared that just as God gave ement to corporal things, so His Vicar gov- d temporal powers, and permitted them to cise their offices. As for the two swords, quoted St. Bernard in the light of Innocent extreme interpretation, and said that the rch (here using the word *ecclesia* in its nar- er, ecclesiastical sense) possessed both, the itual *ad usum* and the temporal *ad nutum*. power of the first, he added, included that ne second and held sway over both spiritual temporal authorities, though for the sake of ncy it shrank from the shedding of blood.[77] emy of Lucca, in the *Libellus sive Tractatus Iurisdictione Imperii et Summi Pontificis*,[78] that the Pope could almost be called *anima di* (though this title was more properly ap- l to Christ); as God's vice-regent he was the ce of all earthly authority. Ptolemy com- d the relationship between Pope and Em- r with that between soul and body, the former g the source of motion and activity, the latter, *num anime ad operandum*. Though offices ne Church were distinct, he said, all govern- and every act depended *a papa per Christum*. ccepted Innocent IV's estimate of his author- s Vicar of Christ and observed that Christ bestowed on the Pope *all* the authority which possessed. Ptolemy also accepted Innocent theory of the Donation of Constantine: that as not a gift but a restitution. Christ had n, according to this theory, not only pontifical also regal power to Peter. Before Constan- submission to Christ's Vicar, his rule was ly a tyranny; the legitimacy of the empire was contingent upon the Pope's authorization of its power.[79] These arguments are characteristic of the extreme hierocratic theory. What is Remigio's attitude to them?

As we have already noted, he does not contradict their fundamental postulate, the principle of the *reductio ad unum*. The whole Church (taken in its wider sense) is one body and must therefore have one head: *alias corpus esset monstruosum*. Christ is the true head but He is separated from the body by His physical absence, and besides, strictly speaking, Christ the head is not of the same species as the body. Therefore the Pope is head in His place, the *unus summus homo* to whom all men are subject, *tanquam caput coniunctum eorum*.[80] From his authority the authority of secular rulers is derived, and his jurisdiction extends even to them *ratione delicti vel defectus iudicis principalis*, but not *directe et principaliter*.[81] For Remigio, this is a very important distinction. Of course the ecclesiastical power is preeminent; its preeminence is established by authority (by Scripture, by the writings and examples of saints, by the words and deeds of popes, by the admissions of ancient emperors like Constantine, Justinian, Charlemagne, and Otto, and of modern rulers as well, and even by those pagans who are known to have shown reverence for their own priests or for Christian prelates); its preeminence may also be proved by reason. At the same time, papal power over temporalities cannot be immediate *in preiudicium iuris alieni*, except in the cases already noted,[82] since this view has been affirmed by both canonists and civilians and even by the popes themselves; it is also attested by custom and by reason. Moreover, the same arguments which demonstrate the superiority of one power to the other (for example, the disparity between their ends, their instruments, their causes, and the authority and *status* of their ministers) may also be used to show the essential distinction between them.[83]

Aegidius Romanus, *De ecclesiastica potestate*, I, viii; xiii; III, iv (esp. pp. 28, 90, 115, 166), ed. R. ., Weimar, Böhlaus Nachfolger, 1929. For the al canonistic doctrine of the two swords see er's analysis, De ecclesiae potestate coactiva materioud Magistrum Gratianum, *Salesianum* **4**: 2–23, , 1942. Stickler says that for Gratian they repreot the ecclesiastical and secular authorities but the two forms of ecclesiastical coercive power, ual and material. Maccarrone, "Potestas directa," says that it is in this sense that Bernard's words *Consideratione*, IV, iii should be understood. The were later, however, used more generally. For ent IV's view see Winkelmann, E., *Acta Imperii* **2**: 698, Innsbruck, 1885.

d. M. Krammer under the title *Determinatio compsa de iurisdictione imperii*, Hannover and Leipzig, 1909. (*Fontes iuris germanici antiqui in usum rum, Mon. Germ. Hist.*).

[79] *Ibid.*, vi, vii, xv, xxvi (esp. pp. 17–18, 33–34, 51). On the Donation, *cf.* Henry of Cremona, *De potestate papa*, ed. R. Scholz, *Die Publizistik zur Zeit Philipps des Schönen und Bonifaz' VIII*, 467, Stuttgart, Enke, 1903, who also adopts Innocent's view, and the Pope's own words in *Aeger cui levia* (in Winkelmann, *op. cit.* **2**: 696), discussed by Graëfe, F., *Die Publizistik in der letzten Epoche Kaiser Friedrichs II*, 218–219, Heidelberg, Winter, 1909.

[80] See above, notes 75 and 76.

[81] *Contra falsos ecclesie professores, C*, 161r, 164r.

[82] *Ibid.*, 161r.

[83] *Ibid.*, 162v.

Some of the greatest popes have stated this principle clearly. It is a significant indication of Remigio's position that he quotes no popes on this subject after Innocent III and that he seems to regard Innocent as an effective, even if unwilling, opponent of the hierocratic theory.[84] After citing, together with numerous other pontifical canons, the great decision *Per Venerabilem* containing Innocent's refusal to legitimatize the Count of Montpellier's children and thereby to infringe the legal rights of the Count's overlord, the King of France, Remigio remarks rather ironically that there is particular force in the fact that prelates are sometimes constrained, like the high priest Caiaphas,[85] to speak the truth even though it runs counter to their own interest. These prelates should be listened to because of their great learning in the laws. Did not Innocent once declare that in the whole world there was only *unus clericus et dimidius*, maintaining the *unus* to be himself?[86]

Remigio also attempts to meet the specific arguments emphasized by such hierocrats as Giles and Ptolemy. He admits, for example, that both swords are in the possession of the Church, but *aliter et aliter, directe et indirecte, principaliter et ex consequenti*, one *ab ecclesia* and the other *pro ecclesia*, inasmuch as the Emperor is a minister of the Church and temporal things are ordered towards spiritual.[87] Elsewhere he quotes those other words of St. Bernard to Pope Eugenius, "In criminibus, non in possessionibus, potestas vestra . . . In hiis successisti non Petro sed Constantino."[88] Remigio concedes that the spiritual authority may be compared to the soul and the temporal to the body, and that the latter receives movement from the former. But some things are given that cannot be retained; a bishop, for example, may confer benefits which he is unable to keep for himself; the operation of the secular sword must be given to the temporal ruler, though through the authorization of the ecclesiastical power.[89] In regard to the other threadbare argument from analogy, that the Pope grants power to secular princes as the sun bestows light on the moon, he asserts this to be symbolic rather than argumentative theology; moreover, he calls the figure *directe pro nobis*, since it is the moon and not the sun that presides directly over the night of temporal affairs.[90]

Remigio's analysis of the title *Vicarius Christi* is especially interesting. Referring to Innocent IV's statement that Christ would not have shown Himself to be a solicitous *pater familias* if He had not bestowed on Peter "full power over all men": the Dominican asserts that this is rather a proof of divine prudence. A representative of an authority can never be expected to possess all the power of that authority, and in this case Christ did not give Peter full jurisdiction even over spiritual things (for the Pope cannot create a new article of faith or institute a new sacrament or confer the effect of a sacrament without the sacrament itself); much less did He give him full jurisdiction over temporal things. Such restraint is for the good of the Church, to keep it from being corrupted by material riches.[91] When

[84] Remigio's attitude might be cited in support of the modern estimate of Maccarrone, *Chiesa e stato nella dottrina di papa Innocenzo III, Lateranum* N. S. 6 n. 3-4, Rome, Facultas Theol. Pont. Ath. Lat., 1940.

[85] Who prophesied that Jesus should die for the nation. (John 12: 49–51.)

[86] *Contra falsos ecclesie professores*, C. 162r: "Ex dictis igitur ipsorum paparum apparet quod papa non debet se de temporalibus que pertinent ad iudicium seculare intromictere principaliter et directe. Efficatia autem istius pro bonis ex quadruplici parte apparet, quasi ipsa veritate cogente, eos sic dicere: tum quia locuntur contra auctoritatem suam; tum quia non est eorum consuetudo sua iura velle diminuere sed potius augere, sicut apparet per multas decretales imperatoribus missas; tum quia peritissimi in iure sunt ut plures ex eis, maxime Innocentius tertius qui fertur dixisse quod in toto mundo non erat nisi unus clericus et dimidius, se unum clericum vocans; tum quia ipsa auctoritas facit verum asserere, sicut legitur de Caypha Io. xi quod 'cum esset pontifex anni illius prophetavit' nesciens quid diceret."

[87] Ibid., 161r–v.

[88] Ibid., 161r.

[89] *Ibid.*, 162v–163r. Laurentius Hispanus advances a similar argument. See Mochi Onory, S., *Fonti canonistiche dell'idea moderna dello stato*, 223, Milan, Univ. Catt. del Sacro Cuore, 1951.

[90] *Ibid.*, 164r: "Dicendum est enim de omnibus figuralibus generaliter quod ex hiis non potest accipi coactiva probatio, quia simbolica theologia non est argumentativa, ut dicit Dyonisius in epistola ad Titum. Et nichilominus dicendum quod licet luna accipiat lumen a sole, tamen principaliter et directe preest nocti, per quam dicuntur significari carnalia. Non autem hoc convenit soli. Unde figura est directe pro nobis." *Cf.* John of Paris, *Tractatus de potestate regia et papale*, 218, ed. Leclercq, J., *Jean de Paris et l'ecclésiologie du xiii^e siècle*, Paris, J. Vrin, 1942, who makes the same statement, and Dante, *Monarchia*, III, iv, who follows the same general line of argument.

[91] *Ibid.*, 163v: "Et propterea dicendum quod hoc Christus fuerit dominus temporalium, tamen non vicario suo pape committere istud dominium, ut scibat magis spiritualibus posset intendere ad quod ecclesia principaliter ordinatur. Unde hoc non fuit indigentie vel insipientie sed diligentis prudentie, quia in hoc vicarii et commisse sibi familie. Unde ipse ad hoc

stantine made his Donation a voice was heard
n the sky declaring "today poison has entered
God's Church."[92] In another treatise, his
mentary on the Song of Solomon, Remigio
ares that the modern church has suffered more
n its anxiety for temporal wealth than the
nitive church suffered from the persecutions
agan tyranny.[93]

he Dominican makes every effort to reduce
most celebrated examples of papal interven-
in secular affairs to the ground of *ratio pec-*

He says that the last Merovingian was cor-
as well as weak, and therefore it was fitting
Pope Zacharias to transfer the kingdom of
ce to the Carolingian line. The deposition
rederick II was justified on account of his
rious crimes. The translation of the empire
n the Greeks to the Germans was made be-
e of sin.[94]

emigio asserts, too, that secular government
its own independent reason for existence.
regimen of the Pope is supposed to be ordered
he theological virtues, the *regimen* of princes
olitical and human virtues. The former are
urse superior to the latter, but as the instru-
s are distinct, so also are the powers.[95] In
ion, even though Augustine asserted that
ical virtue without theological is false, and
fore secular authority without ecclesiastical,
must not be taken too literally. They are
false by comparison, as silver is rejected in
of gold. Yet political virtues are more
tly related to human acts, considered as such,
than theological, just as the empire directly governs human affairs.[96]

But Remigio makes the momentous concession: "potentia secularis oritur ab ecclesiastica."[97] When the empire is vacant, and a secular judge is lacking, then the ecclesiastical authority has the right to fill the vacancy. He gives a sweeping list of occasions when papal intervention is legitimate; among them are cases in which justice is denied or delayed, cases of papal privilege, rights authorized by custom in certain lands and causes, and discretionary applications of papal power not evidently sinful or contrary to faith. Just as God can set aside the laws of nature to work miracles, so the Pope may set aside the ordinary course of human law.[98] It is Remigio's belief that, though the Pope should, whenever possible, permit the secular authority to exercise its functions without interference, in the last analysis authority and those functions arise from and depend on him.

Perhaps it is fair to say that Remigio was not bold enough or consistent enough in his thinking to establish a really tenable theory of the limits of papal power. He avoided such controversial solutions as the one advanced by his contemporary John of Paris, that really formidable opponent of the hierocratic theory. John started from the position that the natural needs of man required political authority just as the supernatural needs of man required ecclesiastical authority, and this was also the view of Remigio. Yet John went on to maintain that their jurisdictions were as distinct as their purposes, and to deny that one needed to be set in motion by the other. The Pope did not receive his sword from the Emperor nor the Emperor his sword from the Pope. The secular prince was not the minister of the Pope but the minister of God. There were kings in France before there were Christians and the royal power came not from the Pope but from God and the people.[99] Pope Zacharias, for example, did not depose the King of the Franks but approved his deposition by the barons. Constantine did not bestow the empire on the papacy but only the city of Rome together with certain provinces, and even this limited act was invalid.[100] These arguments (like the similar arguments in Dante's *Monarchia*) rested on the principle that the ori-

lendum, licet esset dominus cunctorum, voluit uti
itate. . . . Et cum esset ditissimus, quia possessor
sorum, voluit uti paupertate. . . . Sed dices, ergo
non potest dici simpliciter vicarius Christi, ex quo
abet totum Christi dominium. Et dicendum quod
equitur. Quantumcumque enim sit plenarie vicarius
i prioris vel abbatis vel episcopi, non tamen propter
ose est prior vel abbas vel episcopus, ut scilicet
totum quicquid potest prior vel abbas vel episcopus.
us autem non solum quantum ad temporalia sed
iam quantum ad omnia spiritualia voluit quod papa
et tantum dominium quantum ipse. . . ." For Inno-
words, see Winkelmann, *op. cit.* **2**: 696–703; for
alysis of them and of Remigio's argument, Maccar-
Vicarius Christi, 124–126, 145–149. Rome,
num N. S. **18**: n. 1–4, 1952.

92 *Ibid.*, 161r–162v. The same legend is referred to by
of Paris, *op.* and *ed. cit.*, 245, and by Manfred in
ter to the Romans, *Mon. Germ. Hist., Const.* **3**:
424.

93 Cod. Conv. Sopp. 362, Biblioteca Laurenziana, 109r;
Paluello, *op. cit.*, 58.

94 *Contra falsos ecclesie professores, C*, 158v, 164v.

95 *Ibid.*, 160r, 162v.

96 *Ibid.*, 163r.

97 *Ibid.*, 162v.

98 *Ibid.*, 164v–165v.

99 John of Paris, *op.* and *ed. cit.*, 194–195, 198–199.

100 *Ibid.*, 218–219, 221–223, 243–247.

gins and functions of political and ecclesiastical authority were distinct. Even though one should aid the other, there was no direct causal relationship between them. Thus John could dismiss the argument from analogy that corporal things must be ruled by spiritual and therefore secular princes by ecclesiastical. The regal power, too, he asserted, was spiritual, ordained to that common good which was life lived according to virtue; the legislator as well as the priest had a *curam animarum*.[101] Remigio himself expressed the same view in regard to the commune when he said that the city offered its citizens intellectual and spiritual growth as well as material necessities, that it was a union of minds more than of bodies.[102] His emphasis on its moral value was the keystone of his exaltation of the commune: its existence was natural in the Aristotelian sense because it made possible the highest human development of its members. It was therefore inconsistent for Remigio in the *Contra Falsos Ecclesie Professores* to accept the body-soul analogy as symbolizing the fact that the secular power received its life and movement from the ecclesiastical authority. Once this concession had been made, it became very difficult to draw the line of demarcation. Remigio's boundary was theoretically unclear and practically very easy for the spiritual power to cross.

This consequence was particularly evident in two sermons, one on the papal crown, for the anniversary of Benedict XI's death, and one on the death of Clement V. In the first, Remigio said that all crowns were subject to the papal tiara, that the Pope possessed both pontifical and regal power, and that he could depose and institute emperors and kings when he judged such action to be opportune (not merely, therefore, as had been maintained in the *Contra Falsos Ecclesie Professores*, on the ground of sin).[103] In the second, he did not eulogize Clement himself, but he exalted the papal office in uncompromising terms, saying that it was universal in three modes: *contentive*, for it contained everything; *possessive*, though it should possess the goods of this earth rather than be possessed by them; *curative*, having care both for spiritual and temporal things. Its authority was universal *vicarialiter*, since Christ constituted the Pope His Vicar *in omnibus;* and *officialiter*, since all offices of whatever dignity and jurisdiction were collected in the person of the supreme pontiff. Remigio quoted the words of Bernard to Eugenius: "Tu primatu Abel, gubernatu Noe, patriarchatu Abraham, ordine Melchisedech, dignitate Aaron, auctoritate Moyses, iudicatu Samuel," and added to them the significant titles, "Tu regno David, tu imperio Octavianus."[104] This may be regarded as a recantation of the views expressed in the *Contra Falsos Ecclesie Professores*, or perhaps, more safely, as an indication of the unsteady foundation on which Remigio's earlier opinions were based.[105]

There is, in fact, a fundamental dichotomy between the two principal portions of Remigio's political theory. His treatment of the problem of faction in the commune was drastic, consistent, and imaginative; his opposition to papal hierocratism was cautious, hesitating, and inconclusive. If he, like John of Paris, had applied the same principles to the *regnum* and the *imperium* that he asserted in regard to the *civitas*, if he had kept the origin and end of all political society firmly within the natural order, his position would have been more coherent. As it is, one must feel that the theories he held about his own commune are in spite of their excesses more interesting, and perhaps more tenable, than those he advanced concerning the larger problem of papal claims to sovereignty over the universal church.

[101] *Ibid.*, 225.

[102] See above, p. 668.

[103] *G*, 378r–379v.

[104] *G*, 379v–381v. *Cf.* 345v and 347r. I hope soon to publish the text of both sermons.

[105] The date of the sermon must be 1314, the year of Clement V's death. The treatise was certainly written before 1304 and probably earlier. See Minio-Paluello, *op. cit.*, 57, n. 5, and Maccarrone, *Vicarius Christi*, 150, n. 128.

THE CORPORATION AS A WELFARE STATE [1]

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(*Read April 21, 1960*)

THE conception of the legal and moral position of contemporary corporate management has undergone a considerable revision over the last few decades. The relative independence of corporate management from strict stockholder control, analyzed by Berle and Means in their now classic book on *The Modern Corporation and Private Property*[2] published twenty-eight years ago, has led to the notion of corporate managers as "trustees." This interpretation has been extensively explored by Berle in his recent book on *Power without Property*.[3]

Their position as trustees, however, requires a little further analysis and specification. The designation best fits the executives of corporations that we are concerned with here. They are chiefly the giant concerns with widely dispersed ownership of equities, and in most instances, operating in several industrial fields in some of which there are only a few competitors, also giants.

MANAGERIAL AUTONOMY AND ACCOUNTABILITY

Now the freedom of corporate managers is only relative. Power without responsibility is always unstable. It is more accurate to view the responsible executives as operating within a sphere of autonomous decision-making, but limited beyond that sphere by counter-pressures or actual veto powers. Limits are set by competitors and customers, by administrative agencies and courts, and notably by suppliers of materials and components, by suppliers of labor, organized or not, and by suppliers of capital, including financial institutions and stockholders. Extreme mismanagement is likely to be met with effective vetoes, exercised in ways appropriate to the interests affected. Competitors may capture markets, and customers buy other brands. Civil and criminal actions may be instituted by public authorities. Suppliers may sell to higher bidders. Workers may quit, strike, or conscientiously withhold efficiency, that is, operate within the tolerable range of sabotage. Financial institutions may refuse loans, and stockholders sell shares in such numbers as to depress the market price, with adverse effects on new stock flotations. In extreme instances they may exercise their reserve rights and through a proxy battle, "throw the rascals out." It seems safe to predict that even the American Telephone and Telegraph Company, the model of independent management, would not dare pass their quarterly dividends for more than a few quarters.

It is, however, because of the autonomous sphere of corporate action that giant corporations depart from traditional theories of business organizations and market behavior in a number of ways, all of which challenge our conception of corporate conduct. Two areas of relative independence provide the basis for some features of internal corporate organization that I want to examine here. These two elements of freedom are the protection against many of the rigors of extreme competition because of a firm position in the market, and the capacity to withhold earnings for self-capitalization, but also for some rather frilly luxuries. The luxuries include indulgence in some welfare practices within the organization, which show up in the financial reports as part of operating costs.

Some features of the internal economics and politics of corporate organization invite the metaphor of "private socialism," or, perhaps more exactly, the "welfare state." Now analogies are always hazardous, and these will not be pursued rigorously. They may, however, give us an initial stance from which to view phenomena otherwise neglected or hidden from view.

Three internal policies that justify the metaphor are the protection of the weak by the strong, a system of benevolent taxation, and the cultivation of collectivism.

[1] This paper is based upon an extensive study of the modern business corporation, part of which was undertaken under the auspices of the Organizational Behavior Project, 1950–1953, at Princeton University.

[2] Berle, Adolf A., Jr., and Gardiner C. Means, *The modern corporation and private property*, New York, Macmillan, 1932.

[3] Berle, Adolf A., Jr., *Power without property*, New York, Harcourt Brace, 1959.

PROTECTION OF THE WEAK

Most large corporations have numerous products and even whole divisions that operate at a loss. This is most understandable in conventional terms if the product is new or the company has entered an unfamiliar market. Risk-taking is an honorable element in business competition. Developments costs may be very high, and the time to recover the investment may be very long, or, risks being what they are, the costs may never be recovered. As a practical matter, however, corporations that invest heavily in research and development do not, and probably could not, allocate costs of development retroactively, or repay those costs out of profits in any precise budgetary fashion. Research units normally do not operate on a cost-accounting or profit-and-loss basis. Rather, research investments tend to be viewed from an averaging point of view, often intermixed with considerable irrational faith. The long-term future when research investments are expected to pay off may in fact never arrive.

The more interesting situations, however, are those in which losses persist but the company does not withdraw from the market. Here several types of rationale are developed, representing varying mixtures of reason and rationalization. The doctrine that most closely approximates economic rationality is that of the "full line." This argument runs that the company's salesmen, calling on distributors, dealers, or ultimate customers must be prepared to offer the full range of related products, including unprofitable items, or the business will go to the competitor who does, if for no other reason than the unwillingness of the buyer to deal with numerous salesmen, accounts and company billing procedures. The suspicion of non-rationality arises, however, because there is little or no evidence that the doctrine is put to experimental test.

A second doctrine justifying continued product unprofitability is that of customer responsibility. This usually involves a product of long standing, which the company may have pioneered, and now feels in some sense obliged to continue even though its competitive position would dictate abandonment to competitors. Incidentally, giant corporations often seem not to understand that there are many markets for which they are too large, because of overhead costs if nothing else, and will continue to produce goods appropriate at one time but no longer profitable. A small company grown large may cling to its original field of operation out of almost unadulterated nostalgic sentiment. And although customer loyalty and distinct company clienteles must be reckoned with, the reckoning may be blithe assumption rather than inquiry or experiment.

A third doctrine is an optimistic one. It runs that the present is bleak but that the future will be brighter for various vague and often unspecified reasons. One consequence of this belief requires special notice. This is the fixing of blame for loss on current operating management. Once such a diagnosis is made, the obvious therapy is to bring in a new manager, preferably one with an outstanding profit record in another division. If the diagnosis is in fact incorrect, a whole succession of managers may have promising careers ruined while the company continues to lose money and the division develops the reputation of an East Siberian labor camp for political prisoners. For others, however, protection has been provided against the cruel consequences of competitive failure.

The interesting point is that these doctrines may be believed, even though lacking objective justification. It must also be borne in mind that a decision to drop a product or product line is likely to require costs in executive time that may motivationally offset the possible saving of other people's money. With so many problems to be faced by executives, not every rational action can reasonably be expected. Some things must be permitted to drift along. Add the genuine human concern for displaced personnel at all levels, and the sufferance of loss becomes more understandable. It seems, indeed, justifiable to conclude that the policies add up to an internal welfare system, whereby the rich (that is, profitable undertakings) support the poor (that is, unprofitable ones).

BENEVOLENT TAXATION

We turn now to a second aspect of the corporation as a welfare state, which I called earlier "benevolent taxation." Some features of corporate organization and conduct need to be noted in order to comprehend this concept. In a large multi-product company many divisions and their managers have "profit responsibility," that is, they have costs of operation but also income that can be allocated to them by ordinary accounting procedures. Other divisions and their managers, including a great variety of "public" (that is, company-wide) services, have only expenses. They must be supported, and this is accomplished

by assessments on the income-producing units, with naturally a consideration of ability to pay. In other words, part of the flow of funds within the corporation operates on principles of taxation and appropriation rather than on market principles. Just as the citizen does not buy governmental services, however much he may pay for them through taxes, the divisional product manager does not normally buy public relations or marketing advice. He may not even like it when offered by persons that his division's profits help to support.

Goods normally move between divisions of a corporation according to market principles, and indeed divisional managers often have the option of buying from other producers. The same is not generally true of services. Even advertising costs are likely to be partially borne by internal taxation rather than by direct outlays by product divisions. This is particularly probable if the advertising contains a considerable quotient of "institutional" messages, that is, on behalf of the company as such.

The rapid expansion of staff services, a contemporary phenomenon in the large corporation, has resulted from decisions at executive level rather than from any internal *market* demand. This expansion is commonly resented by managers with profit responsibility. Whether this expansion is economically justifiable from the point of view of the corporation as a unit in its relations with outside interests is, in the nature of the case, extremely difficult to determine. The very absence of profit accountability means that demonstration of worth to the corporation's goals is difficult or impossible. The point of present interest is that from the standpoint of internal operations the corporation here once more resembles the welfare state.

CONFLICT AND COLLECTIVISM

We turn, finally to various analogies to nationalism and patriotism, and even to some of the milder forms of collectivism, in corporate conduct. Although the pressure for conformity of belief and behavior within the corporation has been exaggerated by recent critics, such as Whyte in *The Organization Man*,[4] there is ample evidence of deliberate cultivation of fairly pure collectivism. This becomes not only understandable but also loses some of its nasty overtones if we recall some of the organizational problems in large corporations.

Corporate citizens comprise a myriad of specific jobs and occupations. They are often widely scattered and only in indirect and formal contact and communication. Personal competition for placement and advancement is an established norm for many job-holders. Loyalities to functional units, to common status and to particular occupations unite groups and divide the whole. Status and occupational loyalties commonly transcend corporate boundaries and extend to brethren in various enemy camps. Representatives of the corporation in its dealings with its significant "publics" must also perforce represent those publics within corporate councils, giving rise to conflict of expert testimony and suspicions of disloyalty. Diverse claims upon the collective resources require strategy, tactics, and ultimate adjudication. (Budget hearings are scarcely occasions for comaraderie or altruistic sacrifice.) Engineers, manufacturers, and marketers vie for identification as of super-ordinate importance for the company's position in the market. Jurisdictional disputes abound at the frontiers of satrapies and small empires. In short, the organization is rife with divisive forces.

What holds the organization together besides the bribery of employment? Perhaps, for many, no other answer is needed, but corporate behavior indicates the felt need for collective goals and collective identification. This is promoted in many ways, ranging from the sublimity of uplifting messages to the ridiculousness of tribal festivals.

The rhetoric of corporate collectivism is instructive. The two human groups most commonly used as representation of the ideal of corporate behavior are the team and the family, neither one of which the corporation strongly resembles. Both, however, represent standards of interpersonal loyalty and even affection, and an identification with the welfare of the group as a whole. Both require a considerable subordination of the individual to group interests. And both imply considerable equality within a framework of able and benevolent leadership. The mandatory use of first names among strangers, not to mention enemies, represents a degree of informal togetherness that not even ideologists of collectivist states have yet seen fit to emulate. It is perhaps the equivalent of "citizen" in Revolutionary France, or "comrade" in Revolutionary Russia.

[4] Whyte, William H., Jr., *The organization man*, New York, Simon and Schuster, 1956.

Corporations do not rely on rhetoric alone to instill loyalty and collective identification. The discipline of "thought control" on matters of political and social policy is difficult to appraise, but no doubt operates with some effectiveness, perhaps more in public (that is, with outsiders) than in private (that is, in the "family"). The goal of minimizing differences in rank and function is also abetted by company "management" courses with a clientele drawn cross-sectionally from the far-flung empire of management, as well as by various recreational "mixers" ranging from Christmas parties through picnics to summer camps.

The corporation is not a state, but it sometimes behaves like one, both because it has some powers of autonomous action, and because its leaders take their responsibilities seriously. Its welfare activities for its citizens of all conditions are of course more extensive than those noted. Many of these other activities come under the heading of "personnel policies," from the physical amenities at the work place to the provision of "public" recreational facilities and private psychotherapy. The same immunity from precise economic accountability that permits the concern for corporate cohesion also permits some solicitude for the individual and his family.

It is of course, easy to be cynical. The scorner's seat is the most comfortable resting place ever invented. Our intentions, however, have been moderately honorable. By adopting a somewhat unorthodox approach we have hoped to understand some distinctly unorthodox behavior.

PROCEEDINGS

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